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THE R. A. F. PENROSE, JR., MEMORIAL LECTURE

SCIENTIFIC MOTIVATION

VANNEVAR BUSH

President, Carnegie Institution of Washington

(*Read April 22, 1954*)

In a general lecture of this sort it is sometimes worth while to attempt to view the current status of scientific effort, to focus upon the crucial points where advance is most rapid, thus to catch a glimmer of where we stand and where we may now be headed, and on this basis to ponder on the motivation which urges us on. This is a difficult thing to do, and no two men will formulate alike even the present status of our understanding. Science is so vast, even when the discussion is confined to the natural sciences, the scene is so diverse in aspect, that such attempts often succeed merely in giving false impressions of simplicity. They are bound to leave out entirely areas from which the most important advances may emerge, they are bound to be colored by personal background, and, in place of a clear summary, they may merely produce more confusion. Yet the attempt may be worth while, if for no other reason, because it is refreshing to back out of the laboratory and the library and gaze out over the landscape. So, with full realization that every man will describe the landscape for himself and emphasize features dearest to him, with recognition that one can only pick and choose, let us take a look.

We need not spend much time with the physicists. They are busy digging into the nucleus of the atom, picking off a new elementary particle every few weeks, speaking a mathematical language which completely bars the unsophisticated from participating in the excitement of their delving. They seem to be having a very good time among themselves, but they have left the rest of us for a time; and we can merely hope they will later come up for air and tell us in a common language what it is all about. From a distance it appears that they may soon quit multiplying particles and tell us more about how one transforms into another, settling down perhaps again to a very few that are truly fundamental. We can sympathize with their difficulty in educating the rest of us; for, as they attack the forces binding the nucleus, their formulations necessarily take mathematical forms which have nothing to do with familiar things—nor should we expect them to. As they proceed there seems to be less of neo-Pythagoreanism among them, a bit less mysticism. One does not so often hear today that indeterminacy introduces chance into nature, provides a place for free will, denies causality. Rather it appears, more simply, merely to set a limit beyond which experimentation can no longer substantiate theory; and this limitation may provide permanent room for alternative formulations. Yet there seems to be no harm in this, and it does render the job interesting. So we may well leave the theoretical physicists to their own devices for a bit; it will be some time before we can understand them. And, when they toss out results which strongly affect other areas of effort, we shall certainly know about it, as we did when they split the atom into substantial pieces. In the meantime, classic physics is being left largely to the applied physicists and the engineers. It has become difficult to tell these two groups apart, just as it is often hard to tell a Democrat from a Republican. And classic physics is bounding forward, giving us new materials and new instruments, clarifying somewhat the solid state, giving birth to wholly new varieties of electronics, even beginning to make some sense out of the weather.

So, let us turn to a field where trends are much clearer and arguments are easier to grasp, where we look at large things rather than little ones. The cosmologists are having a field day; several road blocks have been removed, and they may be about to give us a consistent picture of the origins of the universe, its present status, and its future up to a point. There now seems to be a rapidly widening area of agreement among cosmologists, which is a phenomenon in itself. The red shift is widely regarded as showing a true expansion,

as acceptable evidence that the galaxies are rushing away from us into space with velocities roughly proportional to their distances, rather than that light becomes weary in its long journey, or that cosmic dust nips off minute tributes in energy from the passing photons. The receding velocities are now measured up to 60 per cent of the velocity of light, or 180,000 kilometers per second. There is nothing sure about this theory of an expanding universe as yet; Finlay-Freundlich and Born have recently given pause by a theory which links the red shift with radio waves from space and leaves the astronomers merely measuring a temperature instead of a recession. But the reason why true expansion is now so generally accepted is not so much that insuperable difficulties are presented by alternative explanations of the red shift, as perhaps that the expansion hypothesis is so simple and leads so neatly to intriguing speculations on the mode of origin of the universe. There is now in process of refinement a new scale for the universe; for some of the early measurements have been found to be faulty, which is hardly surprising considering their nature; and the new scale brings the astronomers and the geophysicists nearer together on an estimate of about five billion years for the age of the visible universe. We are now told that, five billion years ago, there was nothing except a mass of nuclear fluid of enormous density and temperature. Then there was a tremendous explosion, and in the first thirty minutes following the explosion the elements were formed. What preposterous temerity—to analyze what happened in a half hour five billion years ago! Yet the hypothesis seems to work, and to yield correct figures for the ratios of elements we observe. Then a bit later, mutually receding clouds of gas condensed to form dust, and the galaxies separated and evolved. Stars were formed as the dust and gas condensed, and continue thus to be formed today. The old Kant-Laplace hypothesis comes to life, as Weizsäcker avoids the pitfall which once wrecked it—its failure to account for observed angular velocities—by taking into account turbulent motions and loss of hydrogen from the evolving system, thus obtaining results which correspond to the composition, spacing, angular velocities, and masses of the planets. Billions of stars are conceived to have been born in billions of galaxies, and it appears that planetary systems may have been frequently formed. There may, indeed, be millions of planets with conditions so nearly similar to those on earth that life such as we know would be possible, although perhaps still improbable. The life history of stars is also falling into line as the atomic energy cycles that maintain their brilliance become sorted out, although we as yet have very little idea why stars occasionally explode.

There are plenty of other mysteries left; for example, how a star can have an enormous, rapidly reversing magnetic field. We do not yet know the distance-speed relations of the galaxies at all well, or the variation of galaxy spacing with distance. It may take some time to find out, for we use the absolute magnitude of a galaxy to measure its distance. The light coming from the most remote galaxies that we can observe left them something like a billion years ago, and so we need to find out more about the variation of total light emission with age before the relations can be pinned down well. Thus we have no present measure of space curvature; neither can we say for sure that the velocities are such as to bar the hypothesis that expansion is a cyclic performance, although it appears as though the galaxies were leaving us, and every other point, irrevocably, and hence as though we were not just repeating over and over an explosion and subsequent collapse. If we do not wish to consider a universe that just bounces occasionally, we can consider, if we will, that space itself began with the primeval explosion, that we have a continually altering curvature of space, that we live in a three-dimensional bubble being blown up in a four-dimensional space. But, since we are three-dimensional beings, with three-dimensional minds, we can hardly expect to proceed at once to go much farther in our speculations. Nevertheless, the cosmologists are now giving us a logical, consistent pattern of the development of the universe. And in doing so they have obligingly allowed us plenty of further time on the earth for our speculations and formulations. We apparently do not need to hurry, except to fend off the disasters which we ourselves may create.

We also have a consistent and logical account of organic evolution on the earth, entirely on a mechanistic basis. Population genetics and its statistical treatment have placed natural selection on its feet so that there is no longer mystery in the gradual shifts or in the origin of new species. The story starts with the beginning of life in the fortuitous appearance of self-duplicating molecules in

the primeval seas—still intangible in the haze of our ignorance of the Pre-Cambrian period—and proceeds from there. More complex forms appear as these special molecules combine into forms capable of seizing materials from their neighbors. There is no longer difficulty with the second law of thermodynamics as evolution thus produces more complex and apparently less probable states, for it is the organism plus its environment that constitutes a system, not the organism alone. Higher species appear, and evolution radiates to fill all available niches in the environment. There is also convergence, widely separated lines heading for the same niche, as when wings appear on pterodactyls, bats, and birds. Overspecialization occurs and species succumb. Intragroup selection sometimes carries characteristics to grotesque and deleterious extremes, and the coupling of genic systems may produce defects at the same time that it produces highly adaptive and favorable characters. When an opening in the environment appears because a group succumbs, it is often strikingly filled from the varied, crowding, evolving life about, and often again left open for a surprising length of time when no adjacent forms are ready to move in. Characteristics which appear to be evolved at random, and actually detrimental, persist in populations, and sometimes prove to be highly adaptive when the species shifts to a new environment, whereupon they become used and perfected. The interaction between environment and the statistical trend of the genetics of a population is by no means entirely worked out, but it appears that we have here the key which opens the door to a valid scheme of natural selection.

Everything I say would be contested in some quarters, for scientists are unanimous on very little. But there is today little tendency to call on vitalist or finalist theories to account for the marvelous variety and intricacy of living things. Thus the process of evolution is conceived to have proceeded mechanically for many hundreds of millions of years, until finally there appeared the primates and man. The evolutionists take the appearance of consciousness during this process in their stride, and the philosophers now seem to concentrate on nuts that are not so tough to crack as this one. So now we have conscious evolution, with man in partial control of his own destiny, with the exercise of a free will, tacitly accepted, appearing out of a wholly mechanistic evolutionary process, and in the common meaning foreign to its fundamental tenets.

Many of the old arguments concerning evolution have dissolved because of the remarkable progress in genetics. Wearying of sorting the genes of fruit-fly chromosomes, the geneticists turned to lower organisms, to bacteria and viruses, to find there the elementary beginnings of a complex system. And they found the same old complexity, with units of heredity, mutations, and all the rest. Moreover, reaching somewhat farther up the evolutionary scale, they now find the hereditary process to be more and more intricate. Multiple gene control of characteristics, cross influence of neighboring genes, genetic factors beyond the genes which control their action, even cytoplasmic inheritance appear. Mutagens, moreover, acting on bacteria, produce mutations over many generations; but this is not the inheritance of acquired characteristics in the old sense; it is direct action of environment on a genetic system long treated as though utterly immune to environmental influence. Specificity of mutagens does not appear strongly as yet, but it may. Gone is some of the old assurance, the dogmatism which could assert the general negative—that the environment has no influence on the hereditary mechanism—because a theory and a system had been constructed, mathematical in its precision, thought to be capable in full development of explaining everything. A set of genes, self-duplicating chemical compounds, occasionally mutating at random, immortal, passed from generation to generation without change and uninfluenced by the host, producing hormones which in turn controlled all development—this was much too simple and too pat. It is somewhat difficult to envisage the way in which such a mechanism could produce a bird with full individual knowledge of how to build a complex nest or to follow a pathway of migration, or a spider that could build a web. The day has not yet arrived when a student can embark upon examination of possible racial memory in a homing pigeon without encountering raised eyebrows. But geneticists are becoming more humble as their systems lead them deeper into intricacy; and this is well, for the story is only in its first chapter, and much hard work lies ahead before another Mendel can create a system adequate for the problems which confront us, many of which we now merely avoid.

All of life in its origin, growth, and hereditary mechanisms seems to revolve about the self-dupli-

cating molecules, the proteins and the nucleic acids. These extraordinary chemical compounds, which can produce replicas of themselves out of the building blocks of their environment, lie at the heart of the great mysteries of life. A full understanding and a capability for adequate manipulation in this field call for a new type of chemistry which can deal with molecules of an atomic weight of a million, for a new type of special thinking, and for new aids to thinking. Some powerful aids are already available, in tracer techniques, chromatography, electronic instruments. Recently there has been a striking step forward, for Crick and Watson have presented a logical explanation of the mechanism by which a nucleic acid can form duplicates of itself in an appropriate medium. Soon we may begin to understand and imitate the formation of the proteins which are vital to us in such an intimate sense.

Biochemistry is making progress on many fronts. The chemistry of muscle becomes more clear. Hormones have been isolated and synthesized. The vitamins, then the enzymes, lead us toward an understanding of catalysis which reaches far beyond combination on a crystal surface. Photosynthesis yields very slowly to attack, but chloroplasts can now be caused to function in vitro; and we may soon be able to isolate chlorophyll itself without altering its form and association in the living cell, surround it with an appropriate set of pigments to interchange the energy of incident photons, and cause it to fix carbon dioxide in a test tube. It is strange that this particular chemical compound, which is central to life on the earth, which appears throughout the plant kingdom in closely allied forms, which evolution provided as the means for seizing and using the sun's energy, resists so long our attempts to watch it work in an artificial environment.

We have long cultivated living tissue in glass bottles; and recently the cultivation of viruses on such tissue has opened up important vistas in the attack on disease. We have proceeded far in the subtle electrochemistry of nerve action. But our distant view of brain action, through encephalography, is very far from providing means for more than a glimmer of understanding of the mechanisms of the brain. Medicine is still largely empirical in spite of scientific progress in biochemistry and physiology; it has new and powerful tools but understands and controls them only vaguely; and its progress toward logical processes, based on sound, far-reaching hypotheses, is slow because of the appalling complexity of its subject matter. Biochemistry is in its infancy—an exceedingly attractive field for men of courage and fertile minds. This is true also of psychology. And where these meet to attempt an attack upon the brain processes of man, there is a situation comparable to that in evolution before Darwin; many of the essential notions have been or are being excavated; but the critical exciting syntheses all still lie ahead.

These are only a few sample areas. Science is expanding exponentially, limited now by the mental capacity it can attract, but threatened also with limitation in other ways, as indeed is all free thought. The evolution of science has some parallels with organic evolution. It is a radiating evolution, producing new species of science almost daily. There is intense specialization to fill niches in the environment. Certain species have specialized to the point where they have lost contact with the main thread—perhaps have lost their capacity for adaptation. Areas have been occupied in which traditional scientific methods really do not apply, where mutation has not yet produced adequate substitutes, and where natural selection acts but lamely.

Civilization generally, science in particular, proceeds because man can store, transmit, and consult the record; because the experience of one generation is available to the next; because an individual can share the knowledge of his neighbor. There has been great progress in transmission, in communication, with telephone, radio, facsimile transmission, television; but this has thus far touched scholarly affairs only lightly. There is progress too in the storing of the record, with microfilm and new methods of printing. But our methods of consulting the record are archaic and essentially unchanged. The library, as we know it, cannot cope with the task before it. Science may become bogged down in its own product, inhibited like a colony of bacteria by its own exudations. There are thousands of journals in physics alone. One of these publishes five thousand pages a year, mathematical, abstruse, difficult. Who can be familiar with it all, and who can find in the great mass in storage the grain of wheat needed for his next step? The pile is mounting daily, science is becoming polyglot, duplication is rife; synthesis, crossing many fields, becomes increasingly difficult and more and more necessary.

As we look over the whole scene of modern science, the impending difficulty is everywhere ap-

parent. We look at any remote, rapidly expanding field, say the chemistry and energetics of photosynthesis; and there are hundreds of able research men and thousands of publications. The key thoughts are necessarily expressed in language which it would take years to master. An individual can now make an advance of such moment that it would have excited the entire intellectual community in the days when science was simple; but the advance becomes known only in an intimate circle and is grossly distorted when it is described outside the circle. The esteem of colleagues, the recognition by men of understanding, which is dear to those whose lives are devoted to unraveling our common mysteries, becomes severely circumscribed. The great awards become artificial and of doubtful value when there are dozens of accomplishments daily, and the truly admirable advances are often beyond the comprehension of laymen. In such a morass how are the great syntheses of the future to be brought to light?

There are a number of things that can be done about it. Aids to man's thinking have proceeded far beyond pencil and paper. Analytical machines are evolving rapidly. Digital machinery, at the moment, holds the center of the stage with analogue machines in the background. Yet one can conceive an analogue machine which could handle the routine of organic chemistry far better than a man can do. It could have a far more extensive and accurate memory. It could manipulate relationships far more rapidly and with greater and more accurate restraints than a human brain. It could even learn by experience if necessary, like Shannon's mechanical mouse, which blunders through a maze by trial and error the first time it is inserted, but the second time proceeds unerringly to the exit, thus showing much better learning capacity than the mouse produced by evolution. Machines for proceeding from an x-ray spectrograph to the parameters of a crystal still need human intervention to supply missing phase relations, but they may be freed of this disability and become powerful instruments indeed. There are rapid selectors, which can examine a thousand items a second and print out the text of the items selected in accordance with a complex code. It is possible, on paper at least, to build a machine which will proceed from item to item by principles of association, as does the brain, without relying on pyramidal indexing. There is no reason why man should not relegate to the machine all those parts of his thought processes which are repetitive or subject to precise formulation. There is every reason why he should do so and should turn over to the machine all similar parts of the job of recording and consulting his scientific record. In fact he must do so if he is to handle at all the mass of data he is creating and proceed into the maze of complexity which every branch of science promises to become in the days just ahead. But the developing of such machines is now everyone's business in general and no one's business in particular. It requires a major effort over many years and great facilities and support. The coding of existing material presents a barrier to progress which has thus far dismayed those who might otherwise have plunged in to attack the difficulty broadly. So we nibble about the edges, and the main central bulky problem remains almost wholly untouched. It is not a job for genius, for pulling rabbits out of hats, but for years of effort by diverse groups that have varied techniques at their finger tips. Of course the problem will ultimately be solved if we proceed down the road of mechanization and do not get involved in atomic war; but science will probably go deeper into the morass of extensive, uncorrelated, essentially unavailable product, before it emerges.

It appears that science needs new methods as it approaches problems which reach beyond the simple relations on which much of its present success has been built. These methods will involve new ways of storing and consulting the record, no doubt. But they will involve also new patterns of collaboration where several highly specialized disciplines, beyond true mastery by any one individual, are essential for full insight. Civilization began when an individual could approach a problem bolstered by the inheritance of a written record and sharing experience with his fellows. We can proceed effectively on many of the paths now open only when we learn to interrelate the thought patterns of allied minds with far more intimacy than is now furnished by books, lectures, or seminars. As this occurs the ways in which a scientist will proceed about his business, the ways in which youth will be trained, the position of science in society, will be altered greatly.

Why do we pursue science at all? What are the motivations of scientists? It is well to examine these from time to time, but especially so as we attempt to consider the trends and the ways in which scientists may be called upon to work together in new relationships.

Much of the motivation is clear and immediate.

We need not consider applied science, conducted for commercial or military reasons. Nor do we need to consider basic science pursued because it may later contribute to profitable applications. There are more important categories. One is the research aimed at bettering man's lot, without thought of gain: the attack on disease, or ignorance, or overpopulation. Here the immediate objective is clear, though the underlying motivation is usually unexpressed. But there is a category of research that has no relation to material advantage; it is fundamental science, directed merely at the extension of man's understanding of himself or his environment, or at the extension of his ability to understand.

The motivation behind much of this kind of scientific inquiry is, no doubt, mere curiosity—that strange characteristic of man which, more than anything else, has led to his ascendancy on our planet, and which drives him still toward the mastery of what may now seem the unknowable. Joined to curiosity, and responsible for some of the most amazing flights of genius, is the same aesthetic urge that leads to great art and music. This is peculiarly the case with respect to some of the more abstruse developments in the fields of astronomy, physics, and mathematics. And many scientists derive their strongest motivation, in one way or another, from religion and carry on their mission by faith.

The question of motivation has been examined since science began; every mature scientist has answered it for himself long ago and requires no elucidation of it from me. But there is a reason why it should, from time to time, be considered anew. The number of youth entering on a career in science today is great, and the interest of the public at large in the work of scientists has grown enormously. What do we say to the young man who is immersed in science, with his whole life wrapped up in it? The new things that he must master, if he will advance with his fellows beyond superficiality to creative activity in a specialized field, are so numerous and difficult that he has little time to spend on the philosophical thinking of the past. He is prone to acquire his philosophical orientation accidentally or casually from his scientific teachers, and in doing so he may easily be misled. For I fear that some of the things that are told him in this respect by scientists are erroneous, logically unsound; yet they are likely to be accepted by him just because they appear in scientific dress.

Science has been enormously successful in its representation of reality. Many subjects, once thought to be beyond its purview, have finally yielded to its attack. It normally recognizes, as it proceeds, that there are things it will never know, things that lie permanently beyond the weak sense of man, however extended by instruments. Yet its scope is the whole panorama of the physical universe and all physical aspects of organic life on earth; and these vast areas of knowledge, it is confident, will one day be subjected to its dominion. And out of this confidence, too vaguely defined, there sometimes grows a degree of scientific arrogance. Scientists forget momentarily the limits which science initially set for itself.

Let us consider a presentation that is being made today very forcefully and convincingly. There is nothing fundamentally new about it, for the same presentation appears throughout the history of human thought; but it now acquires a completeness and elaboration which is highly attractive to those who think logically, or believe they do. In substance the presentation goes as follows:

We find ourselves in a mechanistic universe, riding on a fragment from a primeval explosion, projected into nothingness, destined to plunge through space for a while, and then to cool to utter inertness. On this fragment evolution has occurred, an entirely mechanistic evolution from the chance appearance of reproducing molecules, through a myriad of species, sorted out by natural selection, to the appearance of man. Now man, the highest animal, is destined to ride for a while and then perish. There may be millions of other fragments, with organic life on them, sentient beings, conscious of their presence in a role not of their own choosing, riding also to their deaths.

We need not quarrel with this presentation so far. Its formulation is one of the tasks of science. In it science confines itself strictly to the things that can be measured and recorded. It carefully skirts all questions that are not answerable by its methodology.

But from this presentation of the mechanistic universe some recent writers have gone on to formulate a code of ethics as though it followed in logical consequence. This code, in summary, is very simple. Man controls his destiny; let him so control it as to build for himself a better life. That is good which leads in this direction. The code is laudable enough as far as it goes; but it is incomplete and without a logical base in the facts from which it purports to be derived. For it is

based on a tacit assumption that the mechanistic account of the universe that has been constructed within the accepted limitations of science is in fact a complete account, and a proper basis on which to build a complete ethical code. This is to assert that there is no reality beyond those things which we can measure with a rule or time by a clock and that value can be deduced from a statement of fact. But man's motivations emerge from his entire experience. The seat of ethics is in our hearts, not in our minds. Our ignorance is vast. At every turn, as we reach the boundaries beyond which strict definition and logic, measurement and manipulation, cannot be applied, we are confronted with mystery. Our little minds have carved out a region within which science has proved a guide through the murk, leaving blanks and emptiness, but building a consistent conception. In the few thousand years of our existence the logical powers of the mind have accomplished a synthesis in which we can take pride. But to imply that we now grasp the sorry world entire, that we can now draw final conclusions, is to mistake a first step for a journey. We have a useful formulation, within its realm, but have thus far proved little on which to judge our duty or our mission.

Yet there must be motivations for the moment; we cannot wait until our children perhaps lift the edge of the veil a bit, in order to orient our lives today. Thus there is no fault to find with those who go no further than to consider: here we are; let us build a better world—provided this is stated merely as a working formula. Upon such a simple formula, joined with an equally simple definition of what would be a better world, can be erected an acceptable code of ethics. Such a code has been erected by a large portion of those who work in science. It is at times as fine a code as any that has been adhered to by serious men to guide their lives.

Within limits man now controls his destiny. True, the processes of evolution are still effective, and man's control is as yet feeble. True, he can merely deflect a bit the powerful forces of mechanism and chance which rule his life. But his power is growing, and can be consciously enhanced. By this power let us build a world in which men may live happily. Let us conquer disease and banish the causes which distort men's minds. Let us end war and the pressure of population upon the means of existence, which is one of its causes. The construction of an environment in which there is peace and harmony is worthy of our best efforts. It may enable a little child to be happy and healthy and to avoid the mental distortions which produce distress. In it there will be music and art and a flowering of those attributes of the race which somehow transcend the dull problems of food and shelter. In it man may develop his individuality and rise to true dignity. Let us so build that our children may lead better lives. Our code will be a simple one: that is good which leads toward this consummation.

It is dangerous, however, to regard such a code as logically derived from our scientific attainment. The same logic and the same attainment could equally well support a far different conception of what is good. It does support motivations today which are in stark contrast with those by which we wish to live. Logic alone can lead one toward a code which subordinates all means to their end, means of cruelty and deception to an end of regimentation where the free spirit is at the mercy of a communistic state. The urge to serve one's fellow man is not based on any scientific dogma, and the attempt to give it scientific justification involves a dangerous fallacy. The urge is based upon a deep-seated aspiration of the race, which is its only hope.

The simple creed of service set forth above suffices for the day's work for many a scientist. Yet there is joined with it a deep conviction, a faith if you will, which for many a man furnishes motivation and satisfaction, entirely apart from the current struggle. This is the conviction that it is good for man to know, that striving for understanding is his mission. We are embarked upon a great adventure, and it is our privilege to further it. Even though at times the box that is opened be Pandora's, even though there is both good and evil in what we learn, it is our duty and our calling to extend man's grasp of the universe in which he lives, and of himself. By this process, of beginning to understand, we have made such progress as we have. Though the path be thorny, this is still the way in which we should proceed if we would finally emerge from darkness and strike into the light. For who can tell what we may yet learn? Are we now so wise that we can see the picture whole? A million years is a long time, and it was only yesterday that we embarked upon the great adventure. Who are we to conclude that those things which may be today expressed in ergs or quanta encompass all that we shall ever grasp?

The concrete convictions from experiment have seldom lasted more than a generation or a century. The gap between the now knowable and unknowable is vast, and the borderline may shift in unexpected ways. We may still learn many things, and our immature science is no rock on which to erect a final structure for man's ethical guidance.

Thus, as we build codes, even simple codes for the ordering of the work of the day, we need to supplement and extend, to incorporate the scientific faith that the extension of knowledge is a good in itself. If we build well, our children's children may lead happy lives. That alone, yes, is worthy of our best. They may lead lives of harmony, and mutual respect, and diversity, such as we can only dream of. But they may increase in understanding. They may be wise. Their grasp may include mysteries which now elude us, even some of those which we now believe to be forever beyond our ken. We would build for them. We would build not only so that they may live the good life; not only so that man may develop to the full extent of his organic potentialities; but we would build so that our sons may penetrate the gloom a bit, may increase in wisdom and understanding, and perhaps even begin to realize what the great adventure is all about.

Here is a key to a motivation and code that we can in all honesty place before youth. To those young scientists who derive their motivation from their religion we do not need, as scientists, to speak. To those who have lost this anchor, sometimes through the very intensity of their devotion to science, we would offer a motivation that is worthy of their best efforts. When we lean upon the simple faith that it is man's mission to learn to understand, there is no place for a fatalism, for an urge to live in comfort and enjoy, for tomorrow we die. There is no true logic in codes that would submerge and regiment and destroy man's spirit. We wish to so act that those who follow may be healthy men, unharassed, in decent and dignified relationships, free and individual, and to develop the powers of the mind to the utmost. This is our definition of the "better" though not of the ultimate good, and it is not subject to distortion for the enslavement of man's spirit. The definition does not flow from logic but from faith, even from the simple faith which is exemplified by our own devotion to the advance of science. If we build well our children may indeed think more deeply and more surely.

SOME OBSERVATIONS ON THE NATURE OF THE JUDICIAL PROCESS OF SUPREME COURT LITIGATION

FELIX FRANKFURTER

Associate Justice, United States Supreme Court

(*Read April 22, 1954*)

IF ONE is to talk at all before an audience as learned as this, he had best talk about that of which he is least ignorant. And so I have chosen the topic I have, circumscribed as one in my position is to talk about it. But this is not to be a technical professional paper. What I shall say derives from the assumption that I am talking about complicated and subtle problems to those who are not professionally concerned with them, nor professionally trained to their understanding, and yet feel free to make judgments, because as citizens they are deeply involved in these problems. Broadly speaking, the chief reliance of law in a democracy is the habit of popular respect for law. Especially true is it that law as promulgated by the Supreme Court ultimately depends upon confidence of the people in the Supreme Court as an institution. Indispensable, therefore, for the country's welfare is an appreciation of what the nature of the enterprise is in which that Court is engaged —an understanding of what the task is that has been committed to the succession of nine men.

I said I shall speak "circumscribed" as I am in doing so. I am circumscribed not only by the very limited freedom of speech that his position imposes on a member of the Court. I am no less circumscribed by want of those qualities that are not the normal endowment of judges, nor cultivated in them by training. Those who know tell me that the most illuminating light on painting has been furnished by painters, and that the deepest revelations on the writing of poetry have come from poets. It is not so with the business of judging. The power of searching analysis of what it is that they are doing seems rarely to be possessed by judges, either because they are lacking in the art of critical exposition, or because they are inhibited from practicing it. The fact is that pitifully little of significance has been contributed by judges regarding the nature of their endeavor, and, I might add, that which is written by those who are not judges is too often a confident caricature rather than a seer's vision of the judicial process of the Supreme Court.

We have, of course, one brave and felicitous attempt—Mr. Justice Cardozo's little classic. I have read and reread, and reread very recently, that charming book and yield to no one in my esteem for it. And yet you must not account it as immodesty or fractiousness if I say that the book would give me very little help in deciding any of the difficult cases that come before the Court. Why should a book about the judicial process by one of the great judges of our time shed relatively little light on the actual adjudicatory process of the Supreme Court? For the simple reason that *The Nature of the Judicial Process*[1] derived from Cardozo's reflections while in Albany, before he came to Washington. The judicial business out of which Cardozo's experience came when he wrote the book was the business of the New York Court of Appeals, and that is very different business from the most important aspects of the litigation on which the Supreme Court must pass.

Let me indulge in one of the rare opportunities for the valid use of statistics in connection with the work of the Supreme Court. The Reports of the New York decisions for the year during which Judge Cardozo delivered the lectures which comprise his book show that only about one out of a hundred cases before the New York Court of Appeals raised questions comparable to those that gave him most trouble in Washington. The year that he left Albany for Washington, in 1932, only two opinions out of a hundred in the New York Reports raise the kind of questions that are the greatest concern for the Supreme Court. Cardozo wrote something like five hundred opinions on the New York Court of Appeals. In them he was concerned with matters that would not have been foreign, say, to Lord Mansfield or Lord El-

[1] Cardozo, *The nature of the judicial process*, New Haven, Yale Univ. Press, 1921.

lenborough, and would have been quite familiar to Cardozo's contemporaries on the English Supreme Court of Judicature.

After Cardozo came to Washington, he wrote 128 opinions for the Court during the tragically short period that fate allowed him there. He wrote 21 dissents. Of these 149 opinions only 10 dealt with matters comparable to those which came before him while on the New York Court of Appeals. No one was more keenly aware than he of the differences between the two streams of litigation; no one more keenly alive than he to the resulting differences in the nature of the judicial process in which the two courts were engaged. Let me quickly add that such were the genius and the learning and, perhaps most important of all, the priest-like disinterestedness of his mind, that, even during his few brief years as a Justice, Cardozo became an outstanding contributor to the history of Supreme Court adjudication. What is relevant to our immediate purpose is realization of the important fact that the problems dealt with in Cardozo's illuminating little book, and in two other little books which played on the same theme, derive from an experience in the raw materials of the adjudicatory process very different from those that are the most anxious concern of the Supreme Court of the United States.

It is time for me to be explicit. I am advised by an arithmetically-minded scholar that the Constitution of the United States is composed of some 6,000 words. Not every provision of that document that becomes controversial can come before the Supreme Court for adjudication. The questions that are not meet for judicial determination have elicited their own body of literature. A hint of the nature of such questions is given by their fair characterization as an exercise of judicial self-limitation. This area constitutes one very important and very troublesome aspect of the Court's functioning—its duty not to decide.

Putting to one side instances of this judicial self-restraint, de Tocqueville showed his characteristic discernment when he wrote: "Scarcely any political question arises in the United States that is not resolved sooner or later into a judicial question."[2] Those provisions of the Constitution that do raise justiciable issues vary in their incidence from time to time. The construction of all of them, however, is related to the circumambient condition of our Constitution—that our nation is a federalism. The most exacting problems that in recent years have come before the Court have invoked two provisions expressed in a few undefined words—the clause giving Congress power to regulate commerce among the States and the Due Process Clauses of the Fifth and Fourteenth Amendments.

A federalism presupposes the distribution of governmental powers between national and local authority. Between these two authorities there is shared the power entirely possessed by a unitary state. In addition to the provisions of our Constitution making this distribution of authority between the two governments, there is also in the United States Constitution a withdrawal of power from both governments, or, at least, the exercise of governmental power is subject to limitations protective of the rights of the individual. Of the two types of constitutional provision calling for construction from case to case, the limitation in the interest of the individual presents the most delicate and most pervasive of all issues to come before the Court. For these cases involve no less a task than the accommodation by a court of the interest of an individual over against the interest of society.

Human society keeps changing. Needs emerge, first vaguely felt and unexpressed, imperceptibly gathering strength, steadily becoming more and more exigent, generating a force which, if left unheeded and denied response so as to satisfy the impulse behind it at least in part, may burst forth with an intensity that exacts more than reasonable satisfaction. Law as the response to these needs is not merely a system of logical deduction, though considerations of logic are far from irrelevant. Law presupposes sociological wisdom as well as logical unfolding. The nature of the interplay of the two has been admirably conveyed, if I may say so, by Professor Alfred North Whitehead:

> It is the first step in sociological wisdom to recognize that the major advances in civilization are processes that all but wreck the societies in which they occur—like unto an arrow in the hand of a child. The art of free society consists first in the maintenance of the symbolic code; and secondly in fearlessness of revision, to secure that the code serves those purposes which satisfy an enlightened reason. Those societies which cannot combine reverence to their symbols with freedom of revision, must ultimately decay either from anarchy, or from the slow atrophy of a life stifled by useless shadows.[3]

[2] *Democracy in America* 1: 280, Bradley ed., New York, A. A. Knopf, 1948.

[3] Whitehead, *Symbolism*, 88, New York, Macmillan, 1927.

The Due Process Clauses of our Constitution are the vehicles for giving response by law to this felt need by allowing accommodations or modifications in the rules and standards that govern the conduct of men. Obviously, therefore, due process as a concept is neither fixed nor finished.

The judgment of history on the inherently living and therefore changing applicability of due process was thus pronounced by Mr. Justice Sutherland, one of the most traditionally minded of judges:

> Regulations, the wisdom, necessity and validity of which, as applied to existing conditions, are so apparent that they are now uniformly sustained, a century ago, or even half a century ago, probably would have been rejected as arbitrary and oppressive.[4]

A more expansive attempt at indicating the viable function of the guarantee of due process was made in a recent opinion:

> The requirement of "due process" is not a fair-weather or timid assurance. It must be respected in periods of calm and in times of trouble; it protects aliens as well as citizens. But "due process," unlike some legal rules, is not a technical conception with a fixed content unrelated to time, place and circumstances. Expressing as it does in its ultimate analysis respect enforced by law for that feeling of just treatment which has been evolved through centuries of Anglo-American constitutional history and civilization, "due process" cannot be imprisoned within the treacherous limits of any formula. Representing a profound attitude of fairness between man and man, and more particularly between the individual and government, "due process" is compounded of history, reason, the past course of decisions, and stout confidence in the strength of the democratic faith which we profess. Due process is not a mechanical instrument. It is not a yardstick. It is a process. It is a delicate process of adjustment inescapably involving the exercise of judgment by those whom the Constitution entrusted with the unfolding of the process.[5]

This conception of due process meets resistance from what has been called our pigeon-holing minds which seek to rest uninquiringly on formulas—phrases which, as Holmes pointed out long ago, "by their very felicity delay further analysis," and often do so for a long time. This is, of course, a form of intellectual indulgence, sometimes called the law of imitation. "Traditions which no longer meet their original end" must be subjected to the critique of history whereby we are enabled "to make up our minds dispassionately whether the survival which we are enforcing answers any new purpose when it has ceased to answer the old."[6]

Round phrases cannot give square answers. The law in the Court's keeping is not a system of logical abstractions. It is not enough to accept or reject, or even to qualify, so-called theories or principles. Before we can do that we must know their origin, development and association, "not excluding the special personal equations of their founders and promoters."[7]

In giving expression through law to the demands of the time, the Court is concerned with how men feel—that is, with beliefs or assumptions which in one way or another enter into, when they do not underlie, juristic views and judgments. For instance, in passing upon claims by an Igorot tribesman to property, the Court recognized that the claims were based on long association, calling this "one of the profoundest factors in human thought";[8] in determining the responsibility of a corporation doing business outside of its chartering State, the Court moved out of the realm of fiction into that of business actualities by taking note of the fact that "[m]en's minds had become habituated to corporate activities which crossed state lines."[9]

But a merely private judgment that the time has come for a shift of opinion regarding law does not justify such a shift. Departure from an old view, particularly one that has held unquestioned sway, "must be duly mindful of the necessary demands of continuity in a civilized society. A reversal of a long current of decisions can be justified only if rooted in the Constitution itself as an historic document designed for a developing nation."[10] It makes an important difference, of course, if the validity of an old doctrine on which decisions were based was always in controversy and so did not embed deeply and widely in men's feelings justifiable reliance on the doctrine as part of the accepted outlook of society. What is most important, however, is that the Constitution of the United States, except in what might be called the

[4] *Village of Euclid* v. *Ambler Realty Co.*, 272 U. S. 365, 387.

[5] *Joint Anti-Fascist Refugee Committee* v. *McGrath*, 341 U. S. 123, 162–163.

[6] Holmes, *Collected legal papers*, 225, New York, Harcourt, Brace and Howe, 1920.

[7] See F.[redrick] P.[ollock] in *Law Quart. Rev.* **39**: 285, 1923.

[8] *Cariño* v. *Insular Government*, 212 U. S. 449, 459.

[9] *Neirbo Co.* v. *Bethlehem Corp.*, 308 U. S. 165, 170.

[10] *Graves* v. *O'Keefe*, 306 U. S. 466, 487–488.

skeleton or framework of our society—the anatomical as against the physiological aspects—"was designed for a developing nation." As to those features of our Constitution which raise the most frequent perplexities for decision by the Court, they were drawn in many particulars with purposeful vagueness so as to leave room for the unfolding but undisclosed future.

At this point one wishes there were time to document these generalizations with concrete instances which would help to define the problem and illustrate generalities from which the Court starts and differences of opinion which naturally enough arise in their application. Such documentation would expose divergencies by which common starting points lead to different destinations because of differences in emphasis and valuation in the process of reasoning. They would also shed some light on the interplay between language and thought. Differences in style eventually may embody differences of content, just as a sonnet may sometimes focus thought more trenchantly than a diffuse essay. It must suffice, however, merely to give intimations of the range of issues that are put to the test of the Due Process Clause, scant samples of the range and volume of cases in which due process is involved. Thus, the Court has been called upon to decide whether Michigan may forbid a woman from being a barmaid unless she be the wife or daughter of the owner, though, as we learn from Shakespeare, the alewife, sprightly and ribald, played a role in English social life. May Louisiana make pilotage on the Mississippi an hereditary calling? May New York release children from the public schools in collaboration with sectarian agencies so as to help shepherd children into classes for religious instruction? May Pennsylvania, in sections of the State with a polyglot population, require children to participate in flag-saluting exercises? Is it surprising that in the decisions on each of these questions, there was a sharp division on the Court?

This restriction of due process upon arbitrary exercise of governmental power is found only in our Constitution. The duty of judicially enforcing the indeterminate and undefinable restriction derived from "due process" places upon the Supreme Court a task with which no other comparable judicial body is charged. It is because of our experience that the framers of the other federal constitutions of the English-speaking world did not include such a provision. Neither the Supreme Court of Canada nor the High Court of Australia nor the Supreme Court of India is confronted with the almost intractable problems of judicial review which the Due Process Clauses in our Constitution engender. The Constitution of India, a document of brilliant and skillful draftsmanship, has perhaps the most elaborate provisions safeguarding what the statute abolishing the Star Chamber termed "Liberties of the Subject," 16 Car. I, c. 10. But with full awareness of the course of our decisions, that ardently democratic country decided not to have in its constitution the one provision of our Bill of Rights, the Due Process Clause, which the Court finds—certainly I do—the most perplexing. Fundamental principles of the kind for which shelter is often sought under the Due Process Clause are set forth in the Constitution of India as "Directive Principles of State Policy" which it is "the duty of the State to apply . . . in making laws," but the provisions of which "shall not be enforceable by any court." [11]

The other major source of puzzling problems is the Commerce Clause. With us the Commerce Clause is perhaps the most fruitful and important means for asserting national authority against the particularism of State policy. The role of the Court in striking the balance between the respective spheres of federal and State power was thus adumbrated by the Court:

> The interpenetrations of modern society have not wiped out state lines. It is not for us to make inroads upon our federal system either by indifference to its maintenance or excessive regard for the unifying forces of modern technology. Scholastic reasoning may prove that no activity is isolated within the boundaries of a single State, but that cannot justify absorption of legislative power by the United States over every activity. On the other hand, the old admonition never becomes stale that this Court is concerned with the bounds of legal power and not with the bounds of wisdom in its exercise by Congress. When the conduct of an enterprise affects commerce among the States is a matter of practical judgment, not to be determined by abstract notions. The exercise of this practical judgment the Constitution entrusts primarily and very largely to the Congress, subject to the latter's control by the electorate. Great power was thus given to the Congress: the power of legislation and thereby the power of passing judgment upon the needs of a complex society. Strictly confined though far-reaching power was given to this Court: that of determining whether the Congress has exceeded limits allowable in reason for the judgment which it has exercised. To hold that

[11] The Constitution of India, Part IV, Art. 37.

Congress could not deem the activities here in question to affect what men of practical affairs would call commerce, and to deem them related to such commerce merely by gossamer threads and not by solid ties, would be to disrespect the judgment that is open to men who have the constitutional power and responsibility to legislate for the Nation.[12]

The problems which the Commerce Clause raises as a result of the diffusion of power between a national government and its constituent parts are shared in variant forms by Canada, Australia and India. While the distribution of powers between each national government and its parts varies, leading at times to different legal results, the problems faced by the United States Supreme Court under the Commerce Clause are not different in kind, as are the problems of judicial review under the Due Process Clause, from those which come before the Supreme Court of Canada and the High Court of Australia.

Judicial judgment in these two classes of the most difficult cases must take deep account, if I may paraphrase Maitland, of the day before yesterday in order that yesterday may not paralyze today, and it must take account of what it decrees for today in order that today may not paralyze tomorrow.

A judge whose preoccupation is with such matters should be compounded of the faculties that are demanded of the historian and the philosopher and the prophet. The last demand upon him—to make some forecast of the consequences of his action—is perhaps the heaviest. To pierce the curtain of the future, to give shape and visage to mysteries still in the womb of time, is the gift of imagination. It requires poetic sensibilities with which judges are rarely endowed and which their education does not normally develop. These judges, you will infer, must have something of the creative artist in them; they must have antennae registering feeling and judgment beyond logical, let alone quantitative, proof.

At this point, Judge Learned Hand bears quoting:

> I venture to believe that it is as important to a judge called upon to pass on a question of constitutional law, to have at least a bowing acquaintance with Acton and Maitland, with Thucydides, Gibbon and Carlyle, with Homer, Dante, Shakespeare and Milton, with Machiavelli, Montaigne and Rabelais, with Plato, Bacon, Hume and Kant, as with the books which have been specifically written on the subject. For in such matters everything turns upon the spirit in which he approaches the questions before him. The words he must construe are empty vessels into which he can pour nearly anything he will. Men do not gather figs of thistles, nor supply institutions from judges whose outlook is limited by parish or class. They must be aware that there are before them more than verbal problems; more than final solutions cast in generalizations of universal applicability. They must be aware of the changing social tensions in every society which make it an organism; which demand new schemata of adaptation; which will disrupt it, if rigidly confined.[13]

The decisions in the cases that really give trouble rest on judgment, and judgment derives from the totality of a man's nature and experience. Such judgment will be exercised by two types of men, broadly speaking, but of course with varying emphasis—those who express their private views or revelations, deeming them, if not *vox dei*, at least *vox populi;* or those who feel strongly that they have no authority to promulgate law by their merely personal view and whose whole training and proved performance substantially insure that their conclusions reflect understanding of, and due regard for, law as the expression of the views and feelings that may fairly be deemed representative of the community as a continuing society.

Judges are men, not disembodied spirits. Of course a judge is not free from preferences or, if you will, biases. But he may deprive a bias of its meretricious authority by stripping it of the uncritical assumption that it is founded on compelling reason or the coercive power of a syllogism. He will be alert to detect that though a conclusion has a logical form it in fact represents a choice of competing considerations of policy, one of which for the time has won the day.

An acute historian recently concluded that those "who have any share of political power . . . usually obtain it because they are exceptionally able to emancipate their purposes from the control of their unformulated wishes and impressions."[14] For judges, it is not merely a desirable capacity "to emancipate their purposes" from their private desires; it is their duty. It is a cynical belief in too many quarters, though I believe this cult of cynicism is receding, that it is at best a self-delusion for judges to profess to pursue disin-

[12] *Polish National Alliance* v. *Labor Board*, 322 U. S. 643, 650-651.

[13] Hand, *The spirit of liberty*, 66, 81, New York, A. A. Knopf, 1952.

[14] Richard Pares, Human nature in politics—III, *The Listener*, 1037, Dec. 17, 1953.

terestedness. It is asked with sophomoric brightness, does a man cease to be himself when he becomes a Justice? Does he change his character by putting on a gown? No, he does not change his character. He brings his whole experience, his training, his outlook, his social, intellectual and moral environment with him when he takes a seat on the Supreme Bench. But a judge worth his salt is in the grip of his function. The intellectual habits of self-discipline which govern his mind are as much a part of him as the influence of the interest he may have represented at the bar, often much more so. For example, Mr. Justice Bradley was a "corporation lawyer" *par excellence* when he went on the Court. But his decisions on matters affecting corporate control in the years following the Civil War were strikingly free of bias in favor of corporate power.

To assume that a lawyer who becomes a judge takes on the bench merely his views on social or economic questions leaves out of account his rooted notions regarding the scope and limits of a judge's authority. The outlook of a lawyer fit to be a Justice regarding the role of a judge cuts across all his personal preferences for this or that social arrangement. The conviction behind what John Adams wrote in the provision of the Massachusetts Declaration of Rights regarding the place of the judiciary in our governmental scheme, and the considerations which led the Framers of the Constitution to give federal judges life tenure and other safeguards for their independence, have, I believe, dominated the outlook and therefore the action of the generality of men who have sat on the Supreme Court. Let me recall the Massachusetts Declaration:

> It is essential to the preservation of the rights of every individual, his life, liberty, property, and character, that there be an impartial interpretation of the laws, and administration of justice. It is the right of every citizen to be tried by judges as free, impartial, and independent as the lot of humanity will admit. . . .[15]

Need it be stated that true humility and its offspring, disinterestedness, are more indispensable for the work of the Supreme Court than for a judge's function on any other bench? These qualities alone will not assure another indispensable requisite. This is the capacity for self-searching. What Jacques Maritain said in another connection applies peculiarly to members of the Supreme Court. A Justice of that Court cannot adequately discharge his function "without passing through the door of the knowing, obscure as it may be, of his own subjective."[16]

This is not to say that the application of this view of the judge's function—that he is there not to impose his private views upon society, that he is not to enforce personalized justice—assures unanimity of judgments. Inevitably there are bound to be fair differences of opinion. And it would be pretense to deny that in the self-righteous exercise of this role obscurantist and even unjustifiable decisions are sometimes rendered. Why should anyone be surprised at this? The very nature of the task makes some differences of view well nigh inevitable. The answers that the Supreme Court is required to give are based on questions and on data that preclude automatic or even undoubting answers. If the materials on which judicial judgments must be based could be fed into a machine so as to produce ineluctable answers, if such were the nature of the problems that come before the Supreme Court and such were the answers expected, we would have IBM machines doing the work instead of judges.

How amazing it is that, in the midst of controversies on every conceivable subject, one should expect unanimity of opinion upon difficult legal questions! In the highest ranges of thought, in theology, philosophy and science, we find differences of view on the part of the most distinguished experts,—theologians, philosophers and scientists. The history of scholarship is a record of disagreements. And when we deal with questions relating to principles of law and their application, we do not suddenly rise into a stratosphere of icy certainty.[17]

The core of the difficulty is that there is hardly a question of any real difficulty before the Court that does not entail more than one so-called principle. Anybody can decide a question if only a single principle is in controversy. Partisans and advocates often cast a question in that form, but the form is deceptive. In a famous passage Mr. Justice Holmes has exposed this misconception:

> All rights tend to declare themselves absolute to their logical extreme. Yet all in fact are limited by the neighborhood of principles of policy which are other than those on which the particular right is founded, and which become strong enough to hold

[15] Article XXIX.

[16] Maritain, *Creative intuition in art and poetry*, 114, New York, Pantheon Books, 1953.

[17] Address by Mr. Chief Justice Hughes, *Amer. Law Inst. Proc.* **13**: 61, 64, 1936.

their own when a certain point is reached. . . . The boundary at which the conflicting interests balance cannot be determined by any general formula in advance, but points in the line, or helping to establish it, are fixed by decisions that this or that concrete case falls on the nearer or farther side.[18]

This contest between conflicting principles is not limited to law. In a recent discussion of two books on the conflict between the claims of literary individualism and dogma, I came across this profound observation: ". . . But when, in any field of human observation, two truths appear in conflict it is wiser to assume that neither is exclusive, and that their contradiction, though it may be hard to bear, is part of the mystery of things." [19] But judges cannot leave such contradiction between two conflicting "truths" as "part of the mystery of things." They have to adjudicate. If the conflict cannot be resolved, the task of the Court is to arrive at an accommodation of the contending claims. This is the core of the difficulties and misunderstandings about the judicial process. This, for any conscientious judge, is the agony of his duty.

[18] *Hudson County Water Co.* v. *McCarter*, 209 U. S. 349, 355–356.

[19] Literature and dogma, *London Times Literary Suppl.*, 51, Jan. 22, 1954.

SOME OBSERVATIONS ON THE CASE OF THE UNITED STATES EX REL. SMITH v. BALDI

JOHN BIGGS, JR.

Chief Judge, Third Judicial Circuit of the United States

(*Read April 22, 1954*)

A DEFENDANT charged with murder, with certain exceptions, is tried in a State court. If he is convicted he can appeal via the Supreme Court of the State by certiorari to the Supreme Court of the United States. But if a State court trial was lacking in fundamental fairness, the defendant can petition a judge of the United States for a writ of *habaes corpus,* and if he wins, can obtain a discharge from State custody. The United States courts and judges exercise a kind of supervisory jurisdiction of State court criminal trials. The question presented to them is *not* whether the convicted defendant is *guilty* or *innocent* but whether the proceedings in the State tribunals have been violative of standards of fundamental fairness and decency.

James Colbert Smith of the title or cases concerning him have been before courts of record at least twelve times. Smith's cases were before the Supreme Court of the United States three times. All proceedings respecting Smith came to an end before a lunacy commission which in 1953, pursuant to a new statute,[1] caused Smith to be committed to Farview, a Pennsylvania State institution for the criminally insane, because he was found to be insane and therefore not subject to execution. It is probable that the cost of Smith's cases to the body politic was at least a quarter of a million dollars. Ordinary common sense might have avoided this expenditure and murder as well.

As the facts relating to Smith are developed I think that my hearers, like myself, will wonder about our own social "schizophrenia." Reading the record of Smith's cases I am left with an abiding conviction that thorough soul-searching by each of us would result in substantial improvements in social justice and in the criminal law.

Smith, a negro, was born in a Philadelphia slum in 1925. He was blinded at or shortly after birth. Later he was cured of his blindness. He was the product of a broken home. He had difficulty in learning to talk and, according to his mother, he was five years old "when you could understand him a little bit." When he finally learned to talk he stuttered. His mother said that she knew "he was a funny child." When he was nine years old he was sent to a reform school as a juvenile delinquent. He finally reached the sixth grade of a public school. He enlisted in the army when he was sixteen. At that age he was already an alcoholic, a dope addict, and a homosexual.

In the army he was court-martialed at least three times. He was sullen, aggressive, a liar, and subject to "Nervous Conditions." He was twice confined in psychiatric wards and finally received a discharge "Other than Honorable." Army physicians described him as "in a constitutional psychopathic state" but also as mentally competent. More wildly contradictory findings would be difficult to arrive at.

Shortly after his discharge Smith stole an automobile in New York City. He was brought before a court and was sent to the Kings County Hospital for mental examination. His disorder was diagnosed as schizophrenia, hebephrenic type. He was not tried in New York but was admitted to the Brooklyn State Hospital where the diagnosis of schizophrenia was confirmed. Two months and eighteen days after Smith's admission to the Brooklyn State Hospital he was discharged as "Recovered," the staff having been overruled by the hospital's senior director. Modern treatment of the disease includes insulin or electric shock therapy. So far as the record shows Smith did not receive such treatments.

About three months later Smith was taken at his own request to the Naval Hospital in Philadelphia because he told a policeman he was afraid he might kill someone. He was transferred from the Naval Hospital to the Philadelphia General Hospital and a note on his admission record prophetically stated "he is homicidal." But he

[1] Section 11 of the new Pennsylvania Mental Health Act, 50 P.S. Pa. § 1224.

was discharged eight days later.[2] Apparently no attempt was made to commit this man of admitted homicidal tendencies. Thereafter he was arrested from time to time for various comparatively minor infractions of the law and served sentences but no effort was made by anyone in authority to ascertain his sanity or permanently to restrain him.

Shortly after his last discharge, Smith, twenty-three years old, a schizophrenic of admittedly homicidal tendencies, armed himself with a stolen revolver and shot and killed the driver of a taxicab, Haines, a veteran of the Marine Corps, whom Smith had ordered to take him to a Delaware River pier. Smith was captured at the scene of his crime. In Haines' pocket was a small sum of money. Smith had taken nothing, althought it would seem he must have intended robbery. A more brutal, unprovoked killing is difficult to imagine. Smith promptly signed a confession and admitted the crime. He was indicted, tried, found guilty of the crime of first degree murder—despite a strong showing that he was mentally incompetent—and was sentenced to death.

Some details of Smith's trial and subsequent proceedings relating to him are set out hereinafter. He was arraigned—i.e. required to plead guilty or not guilty to the charge of murder, in the Court of Oyer and Terminer of Philadelphia County. Counsel engaged for him was inadvertently absent. The Judge presiding directed a member of the Philadelphia Bar who had never seen Smith before to instruct him as to his answer. Smith at the direction of his counsel pleaded "Not Guilty" and thereby lost an opportunity to have his sanity determined *in limine* by a jury under the Pennsylvania law.[3] The lawyer designated by the Court at Smith's arraignment knew nothing of his history of mental illness. This summary procedure has now been reformed. The lawyer privately engaged for Smith then entered an appearance and filed a petition for the appointment of a commission to inquire into Smith's mental condition.[4] But this petition was dismissed, the Court holding that such an inquiry could be ordered only if requested by the superintendent or warden of the jail in which Smith was confined.[5] Dr. Frederick S. Baldi, the Dr. Baldi of the title of this article, then Superintendent of the prison in which Smith was confined, a physician who was not a psychiatrist, concluding that Smith was sane, refused to make such a request. Because of Smith's case the Pennsylvania legislature has amended the Mental Health Act so that now the attorney for an accused may file with the Court a petition for his mental examination which will be acted upon *in limine* irrespective of any such request by an officer of an institution in which the accused is confined.[6]

Smith's privately engaged attorney made every effort to procure evidence relating to Smith's commitment because of mental illness in New York. Lacking funds to effect this, he resigned as counsel but was reappointed by the Court together with another able Philadelphia attorney to represent Smith at his trial. In Smith's petition for *habeas corpus* filed in the United States District Court for the Eastern District of Pennsylvania it was alleged that Smith's counsel, the Assistant District Attorney and the Court of Oyer and Terminer agreed that if Smith would plead guilty *generally*[7] to the charge of murder the records of his mental illness could be procured and that if this raised doubt of Smith's sanity the Court would consider the withdrawal of the plea of "Guilty." Smith alleged that this agreement was acted upon; that he withdrew his plea of "Not Guilty" and that on the same day the plea of guilty was entered the Court found him guilty of first degree murder without having received the records of his mental illness in evidence on his behalf. There is evidence which supports this contention.[8]

The Court of Oyer and Terminer appointed Dr.

[2] See Art. III, Sec. 301, "The Mental Health Act [of Pennsylvania] of 1923," P.L. 998, 50 P.S. Pa. Sec. 41. The same provision was carried over into "The Mental Health Act of 1951," Art. III, Sec. 304, as amended 1951, January 14, 1952, P.L. 2053, Sec. 7, 50 P.S. Pa. Sec. 1164. It is interesting to note that a patient cannot be kept longer than ten days on a self-commitment. The ten-day period runs from the day that the patient makes a written application for a release. It does not appear whether or not Smith had made such an application but it is highly improbable that he did so.

[3] See Act of March 31, 1860, P.L. 427, Sec. 67, 19 P.S. Pa. Sec. 1352.

[4] Pursuant to the Pennsylvania Mental Health Act of 1923, as amended May 28, 1937, P.L. 973, Sec. 1, 50 P.S. Pa. Sec. 48.

[5] See Commonwealth v. Dunn, 47 Pa. D. & C. 685.

[6] 1951, June 12, P. L. 533, Art. III, Sec. 344, as amended 1951, January 14 (1952), P.L. 2053, Sec. 11.

[7] A defendant cannot plead guilty to a first degree murder charge in Pennsylvania. He may plead guilty generally and the burden is then upon the Commonwealth to prove him guilty of *first degree* murder.

[8] See the opinion of the Supreme Court of the United States in Smith v. Baldi, 344 U.S. 561, 563–564, and the dissenting opinion in the United States Court of Appeals for the Third Circuit in United States v. Baldi, 192 F. 2d at pp. 556–557, and the appendix to that opinion at p. 569.

William Drayton, Jr., a psychiatrist, to examine Smith and to report to the Court as to Smith's mental condition. Dr. Drayton reached the conclusion that Smith was mentally ill when he had had himself committed to the Philadelphia General Hospital in the latter part of December 1945. On cross-examination he testified that Smith "had been drinking heavily and he was a schizophrenic," and he stated as his opinion that while in 1945 Smith was mentally sick none the less he had recovered "completely." Within two and one-half years of this time Dr. Drayton himself was committed under the Pennsylvania Mental Health Act to an institution as mentally incompetent and died within a comparatively short time thereafter. But a fact of equal importance is that Dr. Drayton testified that Smith was able to tell the difference "between right and wrong" pursuant to the legal test originally laid down by the opinion of the Judges of England following M'Naghten's case in 1843. The test is embodied in what are now known as the "M'Naghten Rules."

A very brief explanation of the so-called M'Naghten Rules is in order. In 1843 M'Naghten killed Edward Drummond, Prime Minister Peel's private secretary, believing him to be Peel, because he thought that Peel had persecuted him. M'Naghten was tried for murder at the "Old Bailey." The charge given to the jury at the trial by Lord Chief Justice Tindal included the statement that the question to be determined was whether at the time the act was committed M'Naghten knew that he was doing a wrong or wicked act. If the jurors should be of opinion that the prisoner was not sensible that he was violating the laws both of God and man, then he would be entitled to a verdict in his favor; otherwise their verdict must be against him. The verdict was "Not Guilty by reason of Insanity." There was a good deal of public reaction against this verdict. The House of Lords decided to call upon the fifteen Common Law Judges of England to determine what was the law on such cases—possibly with the idea of formulating legislation. It is clear that it was not the intention of the Judges of England to establish definite rules capable of being used apart from the facts of a particular case. The M'Naghten Rules were seized upon eagerly in America and in many of our States they have been reduced to the quick and easy formula of "knowledge of right and wrong." Manifestly the M'Naghten Rules do not take into account or apply or attempt to apply the advance of psychiatric knowledge since 1843.

Under the law of Pennsylvania, as under the laws of at least forty-two of the forty-eight States of the United States, as in England, the sole or the major test of when "insanity" relieves a defendant of criminal liability is whether the defendant knew "right from wrong" when the act was committed. You see fantastic results from the use of the M'Naghten Rules in Pennsylvania. For example, according to precedent these rules are to be applied only during the trial of an accused for the purpose of determining his mental condition at the time the criminal act was committed. If insanity develops after conviction "medical insanity" is sufficient to avoid the death penalty, and the Supreme Court of Pennsylvania has stated that it will not knowingly permit an insane person to be executed. However, in the recent Patskin case the accused was executed in Pennsylvania after a Commission appointed under the Mental Health Act to determine alleged after acquired insanity had made a most comprehensive examination and had reported that Patskin was a schizophrenic of the paranoid type, chronic and incurable, dangerous to himself and others, and a proper subject for commitment to a mental hospital. The trial court was not satisfied with these findings but called the members of the Commission before it to inquire as to Patskin's present knowledge of "right and wrong." This course had had no prior legal precedent so far as I can determine.

This hundred year old conception plagues the law of criminal responsibility. The Rules assume the existence of a ". . . logic-tight compartment in which the delusion holds sway leaving the balance of the mind intact . . ."; [9] the criminal retains enough logic in the tight compartment so that from this sanctuary of reason he may inform himself as to what the other part of his mind, the insane part, has compelled or permitted his body to do. If the sane portion of the accused's mind knows that what the insane part compels or permits the body to do is wrong, the body must suffer for it by way of electrocution or hanging, obliterating both the good and the bad portions, as well as the residence of both.[10]

But the human mind is a whole. It cannot be broken into parts, one part sane, the other insane.

[9] The phrase is taken from S. Sheldon Glueck's, *Mental disorder and the criminal law*, Boston, Little, Brown and Co., 1925. See pp. 169–170.

[10] See the opinion of the dissenting judges in United States v. Baldi, Smith's federal *habeas corpus* proceeding, 192 F. 2d 567–568.

The law, as embodied in the M'Naghten Rules, since it requires the psychiatrist to state whether in his opinion the accused is capable of knowing right from wrong, compels the expert, whose testimony otherwise would be highly useful on the real issue of mental competence, to test guilt or innocence by a concept which has almost no recognizable reality.

It is interesting to note that the Scots, a hard-headed race, have never applied the M'Naghten Rules. The Scottish test of irresponsibility was expressed by Lord Justice-Clerk Inglis in 1863, as follows: "In a strictly legal sense there is no insane criminal. Concede insanity and the homicidal act is not criminal. The act of the insane, which in the sane would be criminal, lacks every element of crime."

I think that England is about to change the M'Naghten Rules. A report was presented by a Royal Commission on Capital Punishment to Parliament by command of her Majesty, Queen Elizabeth, in September 1953. The studies of the Commission as to the laws of other nations reveal that with the exception of those influenced by the English common law, that is to say, the United States and the British Dominions and Possessions (always excepting Scotland), all countries and nations studied by the Commission, employ the medical diagnosis of responsibility as the test for imposing legal responsibility on the criminal. The conclusion of the Commission, with one dissentient, was in part that ". . . the test of responsibility laid down by the M'Naghten Rules is so defective that the law on the subject ought to be changed." [11]

There are also leavening influences at work in Pennsylvania. The Group for the Advancement of Psychiatry, an organization of leading psychiatrists in the United States, acting through its Committee on Forensic Psychiatry, of which a high-ranking Philadelphia psychiatrist, Dr. Philip Q. Roche, is chairman, has rendered a preliminary report to GAP in which, *inter alia*, "Mental Illness" is defined and a statute proposed whereby mental illness may be effectively asserted as a defense. An anomaly is presented in Pennsylvania in that the definition of mental illness set out in the Pennsylvania Mental Health Act may not be availed of as a defense in a criminal trial but may be used to commit a mentally ill or insane person under all other circumstances. The definition of mental illness proposed by the GAP Committee to be used as the test at a criminal trial is as follows: " 'Mental illness' shall mean an illness which so lessens the capacity [of an individual] to use (maintain) his judgment, discretion and control in the conduct of his affairs and social relations as to warrant his commitment to a mental institution." [12]

The proposed statute is as follows: "No person may be convicted of any criminal charge when at the time he committed the act with which he is charged he was suffering with mental illness as defined by this act, and in consequence thereof he committed the act." The proposed statute also provides that if a verdict of mental illness is recorded the court shall immediately commit the defendant to a public institution for custody, care, and treatment.[13]

Very recently the Philadelphia Bar Association has taken steps to create a Medico-Legal Institute for study of the psychiatric problems presented to the bench, the bar, and the medical fraternity. This is a broad advance toward realism.

A few further brief comments and I will have concluded. As I have said, acting pursuant to Section 11 of the new Pennsylvania Mental Health Act, a Commission was appointed to examine Smith following the denial of *habeas corpus* to him by the Supreme Court of the United States. He was committed pursuant to the report of this Commission to Farview. The Commission was comprised of Dr. Edward A. Strecker, considered to be one of the most distinguished psychiatrists in the United States today, Dr. Baldwin L. Keyes, a leading member of the psychiatric profession in Philadelphia, and Bernard G. Segal, Esquire, then Chancellor of the Philadelphia Bar Association. The Commission found Smith to be "chronically insane." Dr. Strecker stated publicly that he had been insane for years. It is interesting to note that Smith ascribed all of his aberrant and criminal conduct to a "hear-do" machine run from City Hall in Philadelphia. He stated that if this machine could be turned off his conduct would be exemplary. Though the Commission repeatedly informed him that his only chance of escaping execution was insanity, Smith throughout his entire examination vehemently maintained that he was sane.

[11] See the Report of the Royal Commission on Capital Punishment, 1949–1953, p. 116.

[12] The substance of this definition was taken from the Pennsylvania Mental Health Act, repeatedly referred to in this monograph.

[13] This recommendation was adopted from the recommendations of the American Institute of Criminal Law and Criminology, October 22, 1914, as reported by the Committee on Insanity, Edwin R. Keedy, Esquire, Chairman.

I should like to leave my hearers in a questioning mood. Smith's cases in a broad sense are an expression of our own social schizophrenia. We desire to punish him who commits a criminal act and are scarcely willing to mitigate the punishment even if he be mentally irresponsible. We insist on adhering to the test of the M'Naghten Rules—did the insane defendant know the difference between right and wrong? If he did, let him be executed! We seem unwilling to admit that "knowledge of right and wrong" does not constitute a realistic test for responsibility. To a large degree the problem is one of semantics and communication. How does one bring home to the layman the gross inefficiency of the rule? The unfortunate accident of the mental illness and subsequent commitment of the very psychiatrist on whom the Court of Oyer and Terminer relied in determining whether Smith should live or die seem to supply a final and symbolic irony to society's inept handling of Smith and Smith's cases. But, two states, Massachusetts and New Hampshire, have already led the way in showing what can be done by willing and socially conscious legislatures in making the law fit facts.

Finally it seems desirable to attempt to draw some sociological conclusions. Did society have any obligation to Smith? I myself am sure of the answer. Many persons would not be so certain. But there is a second question which all of us would answer in the affirmative: "Did society have an obligation to protect itself from Smith?" But society's duties to Smith and to itself were complementary. The pattern of Smith's life, as is the case with most of us, was formed by the time he was sixteen years of age. He was then a criminal, a pervert, and a schizophrenic. The superintendents or wardens of the institutions in which he was confined from time to time could have petitioned for the appointment of a commission to examine and confine him.[14] How society may take some effectual steps to fulfill its obligations to the Smiths of this world and at the same time protect itself is ably presented in a recent article in the *Saturday Evening Post* of March 27, 1954, entitled "Child Criminals Are My Job," by Dr. Rittwagen.

No adequate steps were taken in Smith's case. The brutal murder of Haines followed. And as we look back upon the record that murder seems to have possessed the inevitability of a Greek tragedy. Society did not protect or aid Smith and society did not protect or aid itself. Disregarding all humanitarian considerations a few dollars intelligently expended on Smith in his childhood and boyhood might have saved the expenditure of much greater sums later on. Moreover, the result would have been a sound one for in all probability Smith by the time he was sixteen would have been permanently confined at Farview as he is now.

The influence of the great Commonwealth of Pennsylvania is very broad. Why should it not lead the way in enacting the needed additional statutory reforms and in promoting a sound, integrated medico-legal approach to future potential Smiths? If Pennsylvania establishes these needed reforms, will not other states in this great nation follow her lead?

[14] See Section 308 of the Mental Health Act of July 11, 1923, P.L. 998, referred to in note 7 of the opinion of the Supreme Court of Pennsylvania in Commonwealth v. Ashe, 71 A. 2d 107, 109.

THE MORAL AND INTELLECTUAL RESPONSIBILITIES OF EXPANDING COLLECTIONS

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(*Read April 22, 1954*)

THE printed title of this paper, "The Moral and Intellectual Responsibilities of Expanding Collections" requires perhaps further clarification. It might better be called, "The New Role of the Creative Arts in a Free Society." Art museums in America in the past generation have emerged from a long neglect as the spiritual scrapbaskets of the humanities to a position, for right or wrong, of community leadership. Attracting to their halls and galleries each year more than half the population of the United States, it would seem appropriate for a spokesman for these institutions to bring before this learned and philosophic body the broader implications of the artistic movements of this country, their impact upon general education and the political consequences of recreational facilities in a society in which, in less than two decades, the normal week has been reduced to forty hours —a work week which will probably again be halved by the industrial uses of atomic energy.

I do not wish to pose, like Ruskin, as a Cassandra railing against the inevitable circumstances of our day. Rather I hope to state the mission which I conceive to be that of my colleagues and myself in a world where civilization is embattled. And I further hope to convince you of the academic purple, whose scepticism is no less barbed than that of the economic royalists, that the public collections which are scattered today throughout the forty-eight states of this Union—as well as in Hawaii—offer a new therapy against the insomnia of regimentation and the appalling psychiatric vacuum of private emotional distress. Museums can indeed be considered in a new light, that of intellectual and nervous gyroscopes or stabilizers.

A century ago the English novelist Thomas Love Peacock observed,

> The progress of intellect, with all deference to those who believe in it, is not quite so obvious as the progress of mechanics. The reading public has increased its capacity of swallow, in a proportion far exceeding that of its digestion. Thirty-nine years ago, steamboats were just coming into action, and the railway locomotive was not even thought of. Now everybody goes everywhere: going for the sake of going, and rejoicing in the rapidity with which they accomplish nothing. *On va mais on ne voyage pas.* Strenuous idleness drives on the wings of steam in boats and trains, seeking the art of enjoying life, which after all, is in the regulation of the mind, and not in the whisking about of the body.

These words written a hundred years ago are very apt today, and I therefore accept the challenge of defending the cultural institutions for their zeal in maintaining not the *status quo ante* but a calm disinterestedness in the political and almost theological controversies inherent and implicit in any artistic movement. The museum in my opinion must make available to both the artist and the public the great body of human experience; it must not lead to the extent of advocating one school of thought over another. Like the public library it must give to the full limit of the institution's capacity to do so a fair presentation of current trends, whether modern or conservative, and it must make it as easy as possible for the spectator to grow in artistic stature and to refine his own individual taste according to the enlargement of his understanding.

The art world today is a house divided against itself; the issue being that of artistic freedom. But artistic freedom is very difficult to explain. Because of a fortuitous analogy of words it has been likened in the minds of many artists to academic freedom. I personally do not believe that there is any real analogy because in the case of artistic freedom it is not the liberal fighting against repression by a governing body, but rather it is the inability of the man in the street to grasp the esoteric meaning and content behind the artist's particular vision. The task, therefore, of the museum is to try to narrow the breach that already divides the artist from the public.

The question so often asked, "How free is art?" is almost imponderable. We have become so accustomed to the clichés that art knows no frontiers

and it is the inalienable right of the poet, the painter, and the sculptor to perform in an intellectual vacuum of his own creation, that we have arrived in mid-twentieth century at a concept of artistic freedom which is no longer free and debatably artistic. Just as the late President Roosevelt argued in his plea for the four freedoms that the greatest fear is fear itself, so it can be contended that the most urgently needed freedom in the arts is the freedom from that dehumanization of the creative spirit which seductively passes today for the real thing. Having liberated ourselves from the totalitarianism of the academy which dominated the eighteenth and nineteenth centuries, and fleeing as we do from the pitfalls of the totalitarianism of political ideologies of which we have had such ghastly experience in our own lifetime, we seem to be headed for a new and equally destructive absolutism—that of the semi-detached and unfurnished mind which is rented out by the day and night without reference to the welfare of the neighborhood, and which is utterly secure in the smug conviction that it will not survive the jerry-built houses that surround it. With the essential conduits laid for air-conditioning and electrifying the intellect, tradition, if it is ever wanted, can be turned on by pressing a button.

There is, in fact, a charming legend that is told about a small freshwater college in the Middle West where recently a new concrete walk was laid across the campus. While the concrete was still green, signposts were set up at either end which stated in bold capitals: "This walk is restricted for the use of members of the senior class. This tradition will go into effect next Monday."

A new freedom, then, is required for the creative arts in a free society—the freedom of being unashamed of the past. It is ironical that a generation which chooses to go bareheaded as much as possible should have such a phobia about being accused of being "old hat." Where indeed have gone the great eccentrics and non-conformists of the nineteenth century? the Renoir peering in the window of a Paris bookshop at a reproduction of Giorgione's Dresden Venus and shouting "Un ventre comme ça vaut le sermon du Mont des Oliviers," or the Cézanne who deplored his inability to produce "the art of Poussin, the art of museums"?

In our frantic efforts to free ourselves from the shackles of the academy, we have I think merely fallen into another trap. We have become the victims of the divine right of the extreme left. This has not been so much the result of any ideological conviction as it has been the innate carelessness of the liberal to guard himself against the encroachments of totalitarianism or of that particular brand of democracy which has been described as "tyranny softened by sloppiness."

Taste, conformity, and fashion have been the inseparable companions through the ages. Never was it more noticeable than in the eighteenth century and the Regency. The Honorable Mrs. Pinmoney, a character in one of Peacock's novels, *Melincourt*, uttered the following prophetic words:

> Tastes—they depend upon the fashion. There is always a fashionable taste: a taste for driving the mail—a taste for acting Hamlet—a taste for philosophical lectures—a taste for the marvellous—a taste for the simple—a taste for the brilliant—a taste for the sombre—a taste for ghosts—a taste for the devil—a taste for French dancers and Italian singers and German whiskers and tragedies—a taste for enjoying the country in November, and wintering in London until the end of the dog days—a taste for making shoes—a taste for picturesque tours—a taste for taste itself, or for essays on taste; but no gentleman would be so rash as to have a taste of his own, or his last winter's taste, or any taste but the fashionable taste.

William Blake had earlier protested against a criticism of a fellow artist in the *Monthly Magazine*:

> The taste of English amateurs has been too much formed upon pictures imported from Flanders and Holland; consequently our countrymen are easily browbeat on the subject of painting; and hence it is so common to hear a man say: "I am no judge of pictures," but O! Englishmen! know that every man ought to be a judge of pictures, and every man is so who has not been connoisseured out of his senses.

Once again conformity is undoubtedly the greatest menace of our age; in politics, in women's dress and the standardization of motor cars, in the bad manners of the young—in short, we find it in almost every aspect of our daily life. It is the fashion for the unfashionable, an almost manic desire to share with one another the lowest common denominator and a determination to proclaim as loudly as possible the second hand ideas that we have been sold like so many cases of laundry soap. Since we are timid about our personal opinions, we develop the arrogance of the conviction of the group. We send pictures to exhibitions which do not in any way represent our intimate visions and experiences but which look

like the last picture that took a prize at the last national exhibition and which we have seen reproduced *ad nauseam* in the illustrated magazines. Criticism in literature looks to the collective security of the book clubs and the distribution of box tops. The defense of corporate judgments by selected panels of experts is rapidly destroying that thrilling speculation in individual creative capacity that has distinguished patronage for centuries past from the current taste for organized philanthropy.

The explanation for this leveling process of the American mind was suggested to me by a member of the Society who pointed out that as far back as 1838 Miss Harriet Martineau, in *How to Observe Morals and Manners* had written

> The absence of an aristocratic or sovereign will impels men to find some other will on which to repose their individual weakness, and with which to employ their human veneration. The will of the majority becomes their refuge and unwritten law. The few free-minded resist this will, when it is in opposition to their own, and the slavish many submit. This is accordingly found to be the most conspicuous fault of the Americans. Their cautious subservience to public opinion,—their deficiency of moral independence,—is the crying sin of their society. Again, the social equality by which the whole of life is laid open to all in a democratic republic, in which every man who has power in him may attain all to which that power is a requisite, cannot but enhance the importance of each in the eyes of all; and the consequence is a mutual respect and deference, and also a mutual helpfulness, which are in themselves virtues of a high order, and preparatives for others. In these the Americans are exercised and accomplished to a degree never generally attained in any other country. This class of virtues constitutes their distinguishing honour, their crowning grace in the company of nations.

The question therefore arises as to whether genius and group consciousness can live side by side. For genius it seems has ever been measured by courage and humility rather than by egotism. Are we sure that our society, slogan-ridden and pressurized, provides the most fertile field in which these qualities can flower?

Last year in Portugal I stumbled across a French translation of an ancient Hindu proverb: "Plus le singe monte haut, plus il montre le derrière." I have since learned that in an earthier, more Anglo-Saxon language, Christopher Marlowe said very much the same thing. If this admonition were justified in Elizabethan times, how much more necessary is it to repeat it in our own.

This state of affairs, which many of my colleagues have rebuked me so severely for deploring—as, possibly not playing the modern game in the good old-fashioned modern way—has been rationalized with a variety of five-dollar words, most of them borrowed from the German and from the case histories of psychoanalysts. *Zeitgeist* and *Weltanschauung* are among the especial favorites which depict the subconscious *Lebensraum* in which, both in form as well as function, the *Ego* and the *Id* continue their struggle for self-expression. No one is so ill advised as to imply that totalitarianism has reared its ugly head in this environment; we are asked only to accept the spontaneous rigidity of these formulae as an outward expression of an inward and spiritual grace. Artistic freedom as an issue between the modernists and the conservatives has ceased to be an absolute and is now merely a question of whose ox is gored. The idea of live and let live has disappeared from the current scene; and in our reluctance to think and create in terms of our own experience as human beings we are compelled to become partisans in a religious war defending theologies which few of us understand and in which still fewer of us believe. Both sides have reached the point of exhaustion. That is a good sign, for from the ashes of artistic nihilism a new phoenix will undoubtedly arise. That is the one lesson of archaeology in which we can all believe.

Lord Halifax once said that the gift of prophecy is a long memory and the development of prophecy in the young is surely the ultimate goal of education: to predict the future in terms of the experience of the past. No one can say what will happen; but history and philosophy can pretty well tell us what to avoid. If, by examining the remnants of civilization for himself, the student reaches his own conclusions, then hundreds of years of college teaching shall not have been in vain. The museum stands by to illustrate the texts.

There are in the United States today over two thousand museums of one kind or another, ranging in size from the historic house in Maine to the large municipal galleries such as those of Boston, New York, Philadelphia, Washington, and Chicago. Their aggregate collections total a valuation of something well over two billion dollars, and the total trust funds of these institutions must run into many hundred millions. More than three hundred art museums have been established

in the United States since the signing of the armistice in 1918. I bring these facts to your attention simply because we are rather apt to overlook them. Each of these institutions is a public trust and an effective weapon in our protective arsenal against the overthrow of the civilization of which we are the privileged heirs. The problem of training a new generation of curators and executives to man these institutions is, therefore, a matter of the utmost urgency. You who are educators have got to share with the museums themselves in this responsibility. Too often a form of academic snobbery in our graduate schools has deflected potentially good museum personnel to college teaching in an effort to make the ivory tower more attractive to the young than a career of public service. The situation has now reached almost a point of crisis because of the changing social pattern of this country.

One by one we have seen the private collectors disappear and it has become increasingly more difficult for individuals to amass the fortunes necessary for the creation of the private collections to which we became accustomed in our youth. The latter have, each in turn, and with a few notable exceptions, found their final resting place in a public institution. We must remember that the great art collections of this country were founded on, and built about, fortunes which had been created before the advent of the income tax in 1913. Even as late as the 1920's, income and inheritance taxes, while they constituted a threat to the collector, had not yet succeeded in eliminating him from the scene. An industrialist with an income of $100,000 after taxes thirty years ago was able to meet all his obligations, maintain his standard of living, contributing to philanthropies and charities, and yet have a substantial margin left over with which to acquire a fine picture or a tapestry. Today all this is changed. What important collections are now being formed are paid for not out of income but with accumulated capital. It is a matter of record, although often overlooked, that if a man has, for example, an estate of ten million dollars to be divided among his three children, these three children would each receive a larger final settlement if he would reduce his capital structure from ten million to seven by putting his top three million into works of art which he could give to a public institution. Thus he would make the top layer of his estate tax free.

There seems to be no indication of abatement to this inexorable progress of death and taxes. Rather than to debate the issue, it might be well to consider what the problem may be, once the generation that has accumulated these large capital fortunes has passed on. Collecting will then become the preoccupation of the state and of the non-profit corporations. Personal taste, to be sure, will inevitably be relegated to the authority of the committee but the committee in turn must always have the leadership of the knowledgeable connoisseur, a person who can do for corporate philanthropic entities what the private collector over so many centuries has done for his own pleasure and edification. Not only must he learn to buy works of art with wisdom and discrimination, but he must learn what to accept and what to reject; and he must remember that a very large percentage of the works of art which come to him by gift at the present time are objects for which the donor is taking a tax deduction up to the limit allowed by law, usually at the request of his tax lawyer. The museum official must therefore be on very firm ground before he accepts or rejects a gift when it has such implications with the Bureau of Internal Revenue.

There is another economic factor which is important to bear in mind, and that is the increasing problem of rarity and the difficulties attendant to bringing works of art from Europe to this country. In the past the competition used to be pretty stiff between private individuals who were usually all after the same picture from the moment it first appeared on the London and New York markets. Today that competition to a great extent has been transferred from private individuals to a quite different but equally brisk competition between corporate bodies—museums and galleries often bidding quite recklessly in the auction room for the same thing. Whereas twenty years ago there was seldom a single year in which more than five million dollars' worth in the aggregate of important works of art appeared upon the market, today fewer works of art are turning up; but they are none the less bringing larger and larger prices. The annual total now far exceeds that amount.

The point that I wish to make is that the writing is already upon the wall for the museum profession unless something is done to help develop a new generation of curators and directors. Not so the art dealer. He will flourish like the green bay tree, for he has a ready-made and constant market. He is merely having to redirect his thinking and to attract the representatives of public institutions instead of the private millionaire; and he does

not really have to worry very much over the fact that the private collector is disappearing from the scene. This is a state of affairs that may last for many years to come. In sum, the burden of my song is this: the art interests of the country have become so tremendous and the financial implications involved so burdensome, that there never was a time when it was more necessary to provide a responsible corps of individuals to carry on our work. And, perversely, there has scarcely ever been a time when less attention has been given to their training.

This may seem a digression to you on my part, but let me assure you that it is pertinent to the discussion. The arts face today a very great predicament, the lack of personnel and training on the one hand and their obsession with the issues of academic and artistic freedom on the other. The young museum man is caught on the horns of a dilemma. Questions of personnel can easily be resolved by the application of careful thought. The other issues, however, go far deeper and are issues of the mind and of the heart. Naturally, he must have the moral and aesthetic courage to follow the famous maxim attributed to Voltaire, "I do not agree with a word that you say but I will defend to the death your right to say it"; but, more than that, he must have the courage of his own rather than of other people's tastes and convictions. Thomas Jefferson in his first inaugural address put it a little differently. "Error of opinion," he said, "may be tolerated where reason is left free to combat it." Here lies the responsibility for the preservation of artistic freedom which is shared mutually by the museum and the university. In the one you have the books and the disciplines derived from them; in the other the works of art themselves, mute and unexplained witnesses of man's continuing capacity for greatness. It is not the function of either institution to impose these disciplines, but, rather, to expose them—that is to say, to lay them out on a line for all to see. Integrity, whether artistic or academic, is after all only another name for freedom and it is not a hunting license for new game. It is an attitude, a willingness to accept the past and to build with it for the future. In brief, it is the marriage of inspiration and common sense.

THE STORY OF RESEARCH ON POLIOMYELITIS

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(*Read April 22, 1954*)

TELLING how we have accumulated what we definitely know of poliomyelitis will illustrate many trends in virus work and emphasize some facts that apply to research in general. To Heine goes the credit of first carefully describing in 1840 the disease entity known as poliomyelitis. Much excellent work on this malady was done during the next eighty-eight years. This will be passed over, and the story will be picked up in 1928. At that time it was agreed that poliomyelitis is caused by a virus and that the disease can be experimentally transferred from monkey to monkey. Even though the disease-producing agent was spoken of as a virus, certain distinguished investigators considered it to be a minute micro-organism, called globoid body, capable of multiplication *in vitro* in the absence of living host cells. Furthermore, it was believed that under natural conditions in human beings the nose is the portal of entry and exit for the virus. In addition to this, it was believed that the virus progresses from the nose by way of nasal lymphatics to the cerebrospinal fluid to be distributed through this medium to different parts of the central nervous system. Finally, it was believed that the virus, once it reaches the central nervous system, causes inflammation and edema which in turn choke neurons to death, thus causing muscular paralysis and other signs characteristic of poliomyelitis. Indeed, much of the rapid disappearance of paralysis in early convalescence was attributed to the abatement of edema and to the functional recovery of neurons, injured but not killed.

Subsequently, the above concepts of the virus and of the pathogenesis and pathology of poliomyelitis were shown to be entirely incorrect. It is now known that the agent responsible for poliomyelitis is not an ordinary minute micro-organism, but a virus capable of replication only in living susceptible host cells in a manner different from that used by other types of disease-producing agents. Although it was realized during the early days of experimentation on the disease that the virus travels along nerves, the active agent was believed to utilize lymphatic channels in the spread along these pathways. In the early nineteen thirties, a great deal of interest was taken in what happens when the virus is dropped in the nose of a monkey. It was found that, even though the active agent was placed on the nasal mucosa, the first signs of paralysis usually appeared in the legs or arms. In seeking an explanation for this phenomenon, it was soon shown that the virus first infected nerve cells in the nasal mucosa and then traveled up axons to infect nerve cells in the olfactory lobes. From here the active agent traveled by nerve pathways to different parts of the brain and cord without getting into the cerebrospinal fluid. Furthermore, during the course of these studies it became evident that the first signs of pathological changes occurred in infected neurons, and that dead and disintegrating infected cells induced inflammation with its concomitant edema instead of the other way around as believed in 1928.

For a long time, it was held that all neurons infected with the virus of poliomyelitis were doomed to die. Within the last few years, however, it has been definitely demonstrated that numerous infected neurons, if not too severely damaged, make a complete functional recovery. This recovery of cells from infection, instead of abatement of edema in the surrounding tissues, accounts for much of the rapid disappearance of paralysis seen in the early months of convalescence.

In 1934 it was demonstrated that severing the olfactory lobes prevented paralysis in monkeys when poliomyelitis virus was administered intranasally or intravenously. In 1935 it was found that sprays of alum or picric acid applied to the nasal mucosa of monkeys several hours before the administration of virus would completely protect the animals from paralysis. Shortly after this, weak solutions of zinc sulphate also were found to be effective. These results were consistent and spectacular. They were due to injury or death

of olfactory nerves in the nasal mucosa making them insusceptible to the virus, and through this local insusceptibility of cells, the portal of entry for the active agent into the central nervous system was temporarily closed. Since weak solutions of the chemical substances are not particularly dangerous when applied to the nasal mucosa of human beings, and inasmuch as it was firmly believed that poliomyelitis virus enters man by way of the olfactory nerves in the nasal mucosa, it was but natural that two large experiments in children, one in Alabama and one in Canada, were conducted during epidemic periods in 1936 to determine whether or not nasal sprays of the chemical agents would prevent paralytic poliomyelitis.

The results in children were just as striking and spectacular as were those in monkeys, but they were entirely different. Monkeys were uniformly protected by the procedure, while children received no protection. Following this, investigators had reasons to question the correctness of their notion that virus enters man by way of the olfactory mucosa. In view of this possibility, investigators turned to a study of olfactory lobes of human beings who had died of poliomyelitis. Upon going through the literature, it was soon found that practically no pathologist had studied the olfactory lobes of people who had succumbed to the disease. Finally, a complete study of the olfactory lobes was made on properly collected material, and the results of the investigation revealed in almost every instance no pathological changes in these tissues. If virus had traveled through these organs, distinct evidence of the journey would have been present just as it had always been present in monkeys. The results in the monkeys were perfect, but the experimental pattern did not obtain in man under natural conditions. If, before doing the experiments, somebody had only thought to examine human olfactory lobes, a relatively easy job, the useless attempts to prevent the disease in children by means of nasal sprays would not have been made.

As shown by previous remarks, nasal secretions as the medium for spreading the infection and the nasal mucosa as the portal of entry for the virus attracted the entire attention of most workers. Yet in 1912 and again in 1915 virus was found in the feces of poliomyelitis patients, and in 1911 experimentally contaminated house flies were shown to carry active virus for forty-eight hours Shortly after this, considerable work was done to demonstrate that the disease can be produced in monkeys by feeding virus to them and by allowing contaminated stable flies to bite them. Little or no attention was paid to these findings until the nasal mucosa-portal of entry idea had been thoroughly discredited. In 1937 and 1938 it was again shown that virus is in the feces of poliomyelitis patients, and forthwith many workers confirmed these findings. Thus, very quickly it was recognized that virus is usually in the stools of poliomyelitis patients and often in large amounts. In 1939 virus was found in sewage. Such a finding meant that large amounts of virus were being put out by an infected population and it was obvious immediately that these amounts could not be accounted for only on the basis of paralyzed persons in the population. Upon investigation, it was discovered that many children with minor illnesses and some who showed no evidence of sickness also were excreting virus in their stools. In 1941 it was demonstrated that flies, not belonging to the house-fly species, caught in neighborhoods where cases of poliomyelitis were occurring, carried active virus. This demonstration was readily confirmed and many fly-catching expeditions got under way. The results of the work showed that flies in epidemic areas carry active virus and that flies which have fed on infected feces excrete active virus for a number of days.

In the meantime, investigation of poliomyelitis experimentally induced in monkeys through the feeding of virus was resumed and in some laboratories became the chief endeavor. The results showed that chimpanzees are easily infected and excrete virus in their stools for a number of days. The Philippine cynomolgous monkey also was found to be susceptible to fed virus and is now the monkey chosen for this kind of work. Rhesus monkeys, which were extensively used in earlier feeding experiments and which were satisfactory for other types of investigation, have proved entirely unsatisfactory for this sort of work. The choice of this animal for most of the earlier experiments on feeding, because it was easily obtained, undoubtedly interfered considerably with progress of poliomyelitis investigations.

Since virus is in the stools of people suffering from poliomyelitis, since flies that have fed on these stools carry active virus, and since monkeys and chimpanzees fed the virus become infected, it was but natural that many workers thought that the mode of spread of poliomyelitis in human populations had been discovered. Indeed, one group of investigators exposed bananas to flies in an epi-

demic area and then fed the bananas to a chimpanzee which forthwith came down with poliomyelitis. In the light of what has been said, is it any wonder that huge fly-abatement campaigns got under way immediately wherever the disease broke out? Anyway, they did. There was much garbage-can snooping and airplanes zoomed through the sky spraying mists of DDT over everything. The net result of all this effort was practically zero, and now it is generally accepted that intimate personal contact and not the fly plays the important role in dissemination of poliomyelitis virus in a human population. The virus is excreted in the oral secretions and in the feces of infected persons and undoubtedly is often on the hands or other parts of the body. Even though these parts of the body retain active virus only for a short time after each contamination, they are being repeatedly contaminated, and through intimate personal contact, the kind usually encountered in homes, the active agent is passed from infected persons to susceptibles, entering them by way of the mouth. What happens after the virus enters the mouth and how it eventually reaches the central nervous system to cause paralysis are problems for future solution.

From 1909, when poliomyelitis virus was first discovered, until 1939 it was generally believed that all strains of the active agent are immunogenically similar. Furthermore, until then the monkey was considered the only experimental animal susceptible to the virus. In 1939 the Lansing strain of virus was recovered from a patient's brain at necropsy and was shown to be pathogenic for the cotton rat. Later it was found that white mice also are susceptible. This did not cause much excitement even though the virus was found to be immunogenically different from other strains with which it was compared. For a time no other strains that would go in cotton rats or mice were encountered, but, since neutralization test could be done much easier and cheaper in mice than in monkeys, several workers made surveys for Lansing poliomyelitis antibodies in human populations in different parts of the world. The results revealed that about 80 per cent of adults from all parts of the world surveyed contained antibodies against the Lansing strain of virus. These findings did cause concern, because so few strains immunogenically similar to the Lansing one had been isolated. After all these years it finally dawned on investigators that results of epidemiological studies, including those on second attacks of the disease, might have been vitiated by the fact that several immunogenic types of virus were active in a human population. This state of affairs existed in spite of the fact that hundreds of strains of poliomyelitis virus had been isolated and that the action of many of them had been studied in experimental animals. Such a condition in relation to no other virus disease has ever existed for so long a time. It was well known that there are at least three immunogenic types of foot-and-mouth disease virus and two distinct types of influenza virus. Furthermore, in the meantime, encephalitis had cropped up in different parts of the world and a virus had been isolated from patients in many of them. Investigators were not satisfied to call these agents encephalitis virus and consider them all strains of one type. They promptly did immunogenic and serological studies which showed that they were dealing with at least seven distinctly different immunogenic agents.

Why, then, were workers in the poliomyelitis field content to go along year after year without systematically investigating the possibility that there might be several different immunogenic types of virus? There may have been several reasons. In the first place, many of them were brought up on the idea, without adequate support, that poliomyelitis is one disease caused by one immunogenic type of virus. Then, when this idea ceased to be as definite as it once was, no worker or group of workers wanted to undertake the arduous job of looking into the matter, nor did they have sufficient money to conduct such an investigation properly. Finally, The National Foundation for Infantile Paralysis induced and financed several groups of workers to carry on a thorough and coordinated typing campaign. This took several years and over a million dollars. The time, effort and money were well spent for it is now known that most, if not all, polimyelitis is caused by three immunogenic types of virus, Types, I, II, and III, or the Brunhilde, Lansing, and Leon types. Thus, for the first time, workers were equipped with the necessary knowledge to study properly the epidemiology of the disease, to spot easily any new type of virus that might be encountered, and to proceed logically toward effective vaccines.

Work on poliomyelitis thus far described was arduous and extremely expensive, because, except that conducted on the Lansing strain in mice, all of it had to be done in monkeys and chimpanzees. In other words, there was a great need for inexpensive techniques that would not only speed

up work but make possible different kinds of work. For many years, a number of investigators had sought in vain for a complement-fixation test that might be used in the diagnosis of poliomyelitis and in the identification of strains of poliomyelitis virus. In 1951 partial success was attained when, by adapting a Type II strain of virus to infant mice, it was shown that infected brain tissue from such mice would specifically fix complement in the presence of serum from monkeys surviving a disease caused by a homotypic virus. Thus, for the first time a reliable *in vitro* test to cover part of the poliomyelitis field was devised. This work marked a distinct advance, but a still better set-up had to be discovered before investigators would have the tools essential for the jobs at hand.

In 1949 the poliomyelitis experimental pot really began to boil, and now there are such good tools that a worker has difficulty in deciding which of the many important problems should be attacked first. The boiling started when it was shown that Type II poliomyelitis virus could be grown with ease *in vitro* in cultures of bits of viable tissue taken from human foreskin or human embryos. This indicated that poliomyelitis virus will grow in non-nervous tissues when they are handled in the proper manner outside of an intact body. Then it was found that certain non-nervous tissues from monkeys would also support growth of the virus *in vitro*. Finally, it was clearly shown that, under proper *in vitro* conditions, virus will multiply in fibroblasts and actually destroy them. Progress along these lines has been so rapid within the last four years that it has already been demonstrated that all three types of poliomyelitis virus will grow in tissue cultures made from bits of monkey testicles or monkey kidneys, that such tissue cultures can be used for the isolation of virus from patients suspected of having poliomyelitis, that neutralization tests for the identification and typing of poliomyelitis viruses can be conducted in tissue cultures instead of in monkeys, and that from tissue cultures large amounts of all types of virus can be obtained for complement-fixation and neutralization tests for the evaluation of antibodies against them in human sera. The successes mentioned by no means exhaust the possibilities of what can now be done.

For years poliomyelitis workers have dreamed of a protective vaccine. Many unsuccessful attempts were made to devise one that would protect monkeys. In 1935 two groups of workers, without proper experimental data to warrant their action and at a time when all poliomyelitis was believed to be caused by one immunogenic type of virus, put on extensive vaccination programs in human beings with disastrous results. For a considerable time following this experience, nobody cared to work on the problem. Finally, a new group of workers went at the problem again and showed that vaccines containing inactive virus can be made which will protect monkeys. However, the level of antibody in the blood stream that was calculated necessary for protection against the kind of challenge employed proved to be so high that repeated large doses of vaccine, often in combination with adjuvants, had to be administered. Furthermore, the vaccines contained large amounts of brain material which in itself proved to be dangerous, particularly in the presence of adjuvants. In view of the inherent dangers of these vaccines, no effort is likely to be made to use them on a large scale in human beings. However, recent results of a limited number of tests of them in children clearly indicate, as will become obvious from remarks to be made in a moment, that a level of antibody in the blood stream capable of protecting against paralytic poliomyelitis can be achieved by injections of inactivated virus.

When the poliomyelitis viruses, through the use of tissue cultures, were made available in high titers and in a relatively clean state entirely free from dangerous brain material, vaccination immediately became a pressing problem. A decision had to be made as to whether or not inactivated tissue-culture viruses, with or without adjuvants, administered parenterally, would produce sufficient antibodies to protect against paralysis but not infection. On the basis of results of work on monkeys, many investigators in the past have believed that it would be impossible to achieve effective levels of antibody in the blood stream by vaccination with practicable amounts of inactivated viruses.

The only way to settle the question was to put on experiments with gamma globulin in human populations naturally exposed to poliomyelitis viruses during epidemics. Since there was no inherent danger in such an experiment, a small one was run in Utah three summers ago and two large ones the summer before last in Texas and Iowa. The results of the first experiment were sufficiently suggestive for investigators to realize that previous work in monkeys might not have been planned properly or that the results had been misinter-

preted. Then new work on Philippine cynomolgous monkeys and chimpanzees was undertaken and the findings showed that much lower levels of antibody than previously thought are sufficient to protect against paralysis caused by orally administered active virus. These results are now accounted for on the basis that a viremia, which had been overlooked or ignored previously, occurs early after oral infection of monkeys and that relatively low levels of antibody prevent virus from entering the central nervous system by way of the blood. The finding of viremia in animals and recognizing its possible importance have led to an intensive search for it in man. If it is found there frequently, present concepts of the pathogenesis of the disease in man will have to be altered.

Sufficient results of the experiments on gamma globulin are now known for investigators to be certain that low levels of antibodies will protect against paralysis for approximately five weeks. Only passive immunity, which is relative and not enduring, is obtained in this manner. From these results, however, it can be predicted with a degree of certainty that some sort of vaccine capable of preventing paralytic poliomyelitis in human beings will be available in the near future. The first one to be used on a large scale will probably consist of inactivated tissue-culture viruses with or without an adjuvant which will be administered parenterally. Such a vaccine will be safe. The active immunity may last one or two years, and vaccination probably will have to be repeated at least once after a lapse of time.

The ideal viruses for a vaccine against poliomyelitis would be those similar to the 17D strain of yellow fever virus, a mutant that has lost its disease-producing properties while retaining its antigenicity. Work along these lines is being conducted through the use of tissue cultures, the method employed to obtain the 17D mutant. If all goes exceptionally well, the ideal vaccine will consist of active mutants of Types I, II, and III viruses which will be administered either orally or parenterally. Such a vaccine should induce a mild, but silent, infection leading to a lasting active immunity without endangering other susceptibles in a population. Just how long it will be before the ideal vaccine is available is a matter of speculation.

Progress in poliomyelitis research has not been continuous. In fact, at times we have gone backward instead of forward, and, when we have gone forward, it has been by fits and starts instead of in a smooth, steady manner. At the roots of progress in poliomyelitis research, as in all research, lie proper concepts and adequate techniques. These have been acquired more slowly in relation to poliomyelitis than is the case regarding other viruses. Because of this, many errors have been made and workers have taken part in many wild goose chases. Those who have made the errors, unless they refuse to admit them and to give up false concepts, should not be held in low repute, for not infrequently their mistakes, if recognized, make easier the paths of other investigators. There are times, however, when workers of great scientific repute continue to misconstrue the meaning of their data or will not admit inadequacies in the techniques employed by them. When this happens, progress may be materially impeded and much effort must be expended in tearing down the false edifice before a true one can be built. Thus, "no one has the right to encumber science with premature assertions," for "an erroneous affirmation which has taken a day to construct requires sometimes twenty years to overthrow."

BENJAMIN FRANKLIN'S OHIO VALLEY LANDS

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EMINENCE among men has many bases and varied aspects. Sometimes it may be merely the result of specialization. Generally it is featured by mental keenness, comprehension of data, and grasp of situations. In a public career, the highest eminence involves not merely great capacities but versatility, breadth of interests, and incessant activity. In American history the names of Benjamin Franklin and Thomas Jefferson best illustrate the highest eminence. In the additional matter of cosmopolitanism, that is in being at home in the world of society and politics, Franklin surpassed all others. A definitive biography, the last word on so eminent a figure, is too much to expect. There are too many facets in the life of such a man.

One important and interesting facet of the life of Franklin was his concern in the frontier matters of his times, the "West" of his era.[1] That he was greatly interested in the American frontier is clearly apparent, especially in the second half of his life.

It is a reasonable conjecture that as an apprentice printer and a great reader Franklin was acquainted with New England land speculation and with the New York fur trade. When he reached Philadelphia in 1723 he established residence in a city which from the beginning looked out westward on to the Pennsylvania hinterland and which by 1725 was entering upon trade with Indians on the Ohio. He shortly knew not only the great merchants engaged in the fur trade but also the field agents transacting their business. As owner and publisher of the *Pennsylvania Gazette* he printed items both from the immediate hinterland and from the more remote frontier frequented by fur traders. As a job printer he put out between 1736 and 1762 no less than thirteen Indian treaties.[2] That his interest should be aroused was inevitable. Doubtless it grew with passing years.

The decade 1740 to 1750 saw interest in western matters greatly deepened and enlarged. John Fraser, George Croghan, William Trent, and other fur traders established posts in the trans-Appalachian region. Land speculation and frontier settlement began in the mountains of Pennsylvania, Maryland, and Virginia. The Lancaster Indian Treaty of 1744 marked an important stage in the fur trade, in land speculation, and in settlement. The justly famous Ohio Company of 1747 was a significant feature of the decade, as was French activity on the Great Lakes and along the Ohio River.

The spirit of the frontier seems to have fully gripped Benjamin Franklin as early as 1748. In that year he sent his son, William Franklin, on the trip of George Croghan and Conrad Weiser to the Logstown Indian Conference. He doubtless pored over his son's journal of the trip and in January 1747 he read eagerly Cadwallader Colden's *History of the Five Nations Indians*. His interest in Indian problems and colonial Indian policy quickly became a matter of analysis and proposals.

A trip to the Susquehanna Valley in 1751, attendance at an Indian Conference at Carlisle in 1753, and wide travels in America, as postmaster-general of the colonies, confirmed the ideas and policies of Franklin. He began to call for regulated trade, fair dealing, united colonial action, frontier forts, and forest rangers. His Albany Plan of Union of 1754 was the specific formulation of these ideas and policies. It was unfortunate that neither the colonies nor the mother country was ready for the acceptance of such proposals and policies.

At the suggestion of Thomas Pownall, Benjamin Franklin, in late 1754, put out a small pamphlet entitled, "A Plan for Settling Two Western Colonies in North America." His support of military measures in 1755 and his military activities in 1756 were consistent with his ideas. That by 1756 he had a vital interest not only in frontier defense but also in westward expansion is fully demonstrated in his statement to George

[1] On this large interest, the present writer has an article which it is hoped may be made ready for publication.

[2] *Indian treaties printed by Benjamin Franklin, 1763–1782*. With an Introduction by Carl Van Doren and Historical and Biographic Notes by Julian Boyd, Phila., Hist. Soc. Penna., 1938.

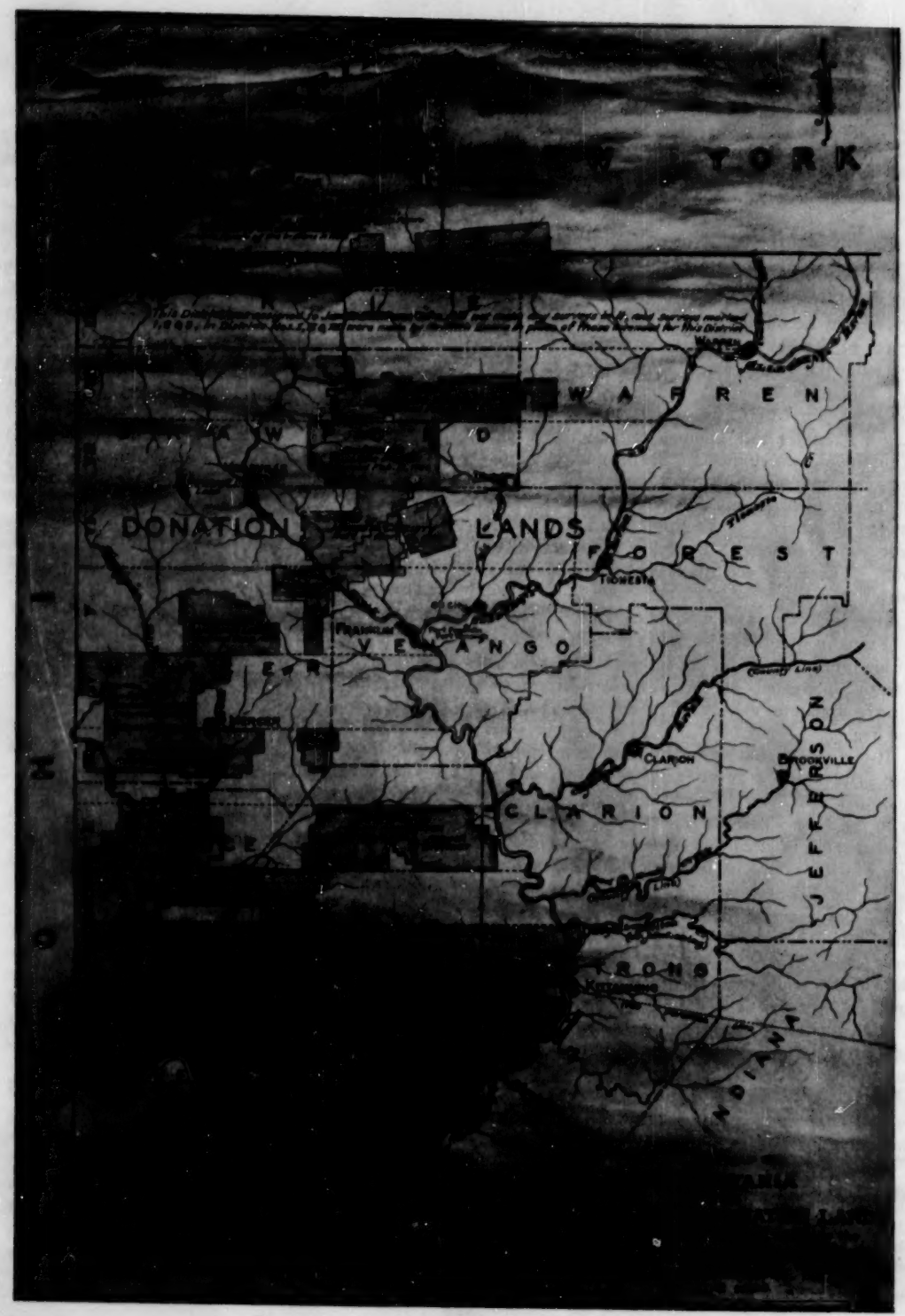


FIG. 1. Courtesy Pennsylvania Land Office Bureau.

Whitefield, "I wish that you and I were jointly employed by the crown to settle a colony on the Ohio. . . . In such an enterprise I could spend the remainder of life with pleasure."[3] The possibility of promoting his ideas and plans for the western country may well have been one of his motives for becoming in 1757 the London representative of Pennsylvania.

That Franklin while in Europe was associated both directly and indirectly with groups concerned with land grants and land speculation is greatly documented[4] and the subject of much historical writing. That his influence, effort, and investment in this matter were without avail is widely known. But that in spite of such failure, he had at the end of his life an extensive land-holding in the Ohio Valley has received little attention. The numerous biographies of Franklin, the printed editions of his writings, and the calendars of his papers throw a minimum of light on this special feature of his general western interests.

In the published writings of Franklin are found only three items about his Ohio Valley property. Two of these are in one document and all three items are lacking in definiteness. In a letter from Philadelphia, April 1786, to Abbé de La Roche, Franklin mentioned Madame Helvétius, asked that his greetings be given to her by the Abbé, an habitué of her salon, and commented on her desire to establish on her estate at Auteuil, an aviary of the famous Kentucky cardinal or redbird. He added significance to this comment by the remark, "She may remember the offer I made her of 1,000 Acres of Woodland out of which she might cut a great Garden and have 1,000 Aviaries, if she pleased." Apropos of this, he stated to the Abbé, in this letter, "I have a large Tract on the Ohio where Cardinals [red-birds] are plenty."[5] Unfortunately the date of the offer is not given nor the location and nature of the "large Tract on the Ohio."

In his last Will and Testament, dated July 17, 1788, Franklin included the statement, "All the lands near the Ohio, and the lots near the center of Philadelphia, which I lately purchased of the State, I give to my son-in-law, Richard Bache, his heirs and assigns forever. . . ."[6]

In comment upon the bequest it seems reasonable to regret that the statement is neither highly revealing nor altogether free from ambiguity. No light is thrown on the exact location and nature of the "lands near the Ohio" and it is not certain whether the clause, "which I lately purchased of the State," refers to the lands near the Ohio or to the lots in Philadelphia or probably to both.

The relation to each other of the three items attracts immediate attention. Were the "1,000 Acres of Woodland," offered to Madame Helvétius, a part of Franklin's "large Tract on the Ohio" of April 1786 and of his "lands near the Ohio," of July 17, 1788? In fact, and somewhat obviously, numerous questions, some major and others minor, arise from the three meager items.[7]

On the item of the offer of "1,000 Acres of Woodland" to Madame Helvétius, careful analysis of available information, extended research for more data and considerable use of imagination have regrettably failed to provide specific answers to questions. Presumably the offer was made while Franklin was in France, 1776–1785. If

[3] July 2, 1756, *The writings of Benjamin Franklin* by Albert Henry Smyth, **3**: 338–340, New York, Macmillan Company, 1907.

[4] See for example, H. S. P. Etting Collection, *Ohio Company papers*. These are available in a poorly edited and badly printed edition, Kenneth Bailey, *Ohio Company papers*, Arcata, Calif., privately printed, 1947.

[5] *The writings of Franklin*, ed. Smyth **9**: 502–506.

[6] *Ibid.* **10**: 493–510.

[7] The attempt to find answers to such questions was supported by Grant No. 1282 from the research funds of the American Philosophical Society. The writer gratefully recognizes his indebtedness to the Society for this grant.

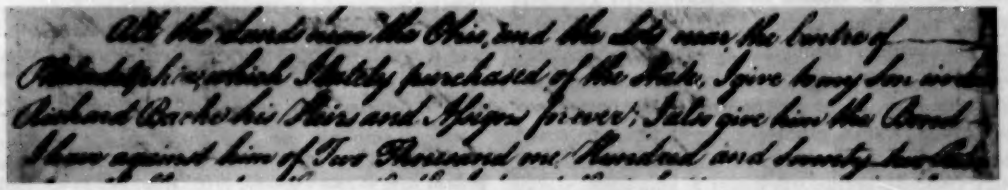


FIG. 2. Portion of Franklin's Last Will and Testament in which Franklin devises the tract of land near the Ohio. Original on deposit with the American Philosophical Society. Courtesy of the Philadelphia Register of Wills.

made before 1780, it must have involved the anticipation by Franklin of a share in the lands of the Vandalia Company, of which he was an active member until 1773 and about which he joined with Samuel Wharton in the Passy Memorial to Congress as late as 1781. If made during 1783–1785, it may have involved either expectations from the land policies of Pennsylvania formulated in 1780–1783, or from some anticipated federal grant for his great diplomatic service, a grant which he fully merited but did not get, though others did. While it is remotely possible that the offer was made to Madame Helvétius by letter after Franklin's return to America in the autumn of 1785, no indication of this was found. But it is not impossible and should not be overlooked that the offer to Madame Helvétius when made, may have been a social pleasantry of which Franklin was both capable and greatly fond. At least for the present and probably for all time this offer to Madame Helvétius must remain an unsolved mystery.

When in the letter to Abbé de La Roche, April 1786, Franklin wrote, "I have a large Tract on the Ohio," it most certainly was not a pleasantry and probably was not altogether a mere anticipation. And when on July 17, 1788 he devised his "lands near the Ohio" to Richard Bache, he was, as will be indicated, dealing with economic, financial, legal, and historical reality. All major and many minor questions about this item have been found clearly answerable.

Five somewhat obvious major questions about the "lands near the Ohio" are: (1) Where were they located? (2) What were their features? (3) How did Franklin obtain possession of them? (4) What use was made of this possession? and (5) What eventually became of them? This report deals mainly with answers to these major questions.

Franklin's "large Tract on the Ohio" could not well have been located in southwestern Pennsylvania, the region occupied today by Westmoreland, Fayette, Washington, Green, and Allegheny Counties. Virtually all the desirable land in this location was taken up before 1785, part of it by Pennsylvania grants, surveys, and patents,[8] but some of it by Virginia titles later validated by Pennsylvania.[9] Examination and analysis of available data on early western Virginia history, involving present West Virginia and Kentucky, and of early Ohio history made it plain that Franklin's tract lay elsewhere. It, therefore, had to be in northwestern Pennsylvania.[10] By the process of elimination there was left no other possibility.

In April 1786 Franklin used the phrase "a large Tract on the Ohio." On July 17, 1788 it is changed to "lands near the Ohio," possibly because of his examination of the surveys of his tract or lands, but possibly because of changed nomenclature in the region. It is not only possible but highly probable that Franklin, on his return to America and in his capacities of president of Pennsylvania, member of the Philadelphia Convention and land speculator, paid much attention to maps and the latest geographical nomenclature.

The phrase "on the Ohio" did not mean in 1786 what it means today. If it did, Franklin's tract in northwestern Pennsylvania would have been land now in Beaver County, Pennsylvania. But such was not the case. Franklin, long before 1757, and more definitively, from his long stay in Paris, must have known well that La Belle Rivière of the French extended from the sources of the Allegheny River to the mouth of the Wabash and that, curiously enough, the French considered the Wabash the main river and La Belle Rivière merely a tributary. He doubtless also knew that the English term "Ohio" at first also included the present Allegheny River. Franklin's tract, therefore, may have been, and in fact was, upon the "Ohio" of the earlier period but upon the Allegheny River of the present day.

The northwestern Pennsylvania lands "on the Ohio" for sale in 1785–1788 were the so-called "Depreciation Lands," lying west of the Allegheny River, a region from 1773 to 1788 within Westmoreland County and from 1788 to 1800 within Allegheny County, but including territory later in Beaver, Lawrence, Butler, and Armstrong Counties.

The establishment of the "Depreciation Lands," was a result of the American Revolutionary War. A Pennsylvania legislative provision of March 7, 1780, promised that, at the end of hostilities, land,

[8] See A. T. Volwiler, *George Chroghan and the westward movement 1741–1782*, Cleveland, Arthur H. Clark Company, 1926.

[9] Alfred P. James, The role of Virginia and Virginians in the early history of southwestern Pennsylvania of the present day, *Western Penna. Hist. Mag.* **34**: 51–63, 1951.

[10] Here considered as that part north of the Ohio River and west of the Allegheny River. Indian claims to the area were ceded only in 1784.

as additional compensation, would be allotted to Pennsylvania soldiers.[11] An act of December 18, 1780, made provision for issuing certificates of depreciation, equivalent to the loss in purchasing power of money paid by the state.[12] No further legislative action was taken for more than two years. It is not impossible that such depreciation certificates were issued in 1782 on the virtual end of military operations and the beginning of negotiations for peace. Such certificates were transferable and may have been in circulation long before April 1786, though no definitive evidence of this seems to have survived. By an act of March 12, 1783, extensive lands west of the Allegheny River were set aside for the redemption of depreciation certificates.[13] Based on a line drawn due west from the mouth of Mahoning Creek, the territory south of the line was to be surveyed into lots of from two hundred to three hundred and fifty acres, which lots were to be sold at public auction in Philadelphia.

But two years elapsed before any surveying was done. The Indians still had, according to the first Treaty of Fort Stanwix of 1768, valid title to the lands under consideration. Not until after the second Treaty of Fort Stanwix with the Iroquois in 1784 and the later Treaty of Fort McIntosh, 1784, with the Delawares, Shawnees, etc., was Pennsylvania fully free to assume control and make distribution of the territory.

Surveying of the "Depreciation Lands" began in the early months of 1785, and lots, duly surveyed and duly returned to the Land Office, began to be sold in November 1785. But only slight information has survived of depreciation certificates, options of purchase, auction sale sheets, sales slips, etc. A great misfortune of the historian and biographer is that generally such papers, commonly known as vouchers, are not usually preserved, whether in private or public records.

That Franklin's "Tract on the Ohio" might be found in the "Depreciation Lands" was first investigated by the consultation of library materials. Franklin's biographies and his printed writings were the starting point. Printed calendars of his manuscripts were carefully scanned. Immediately available county histories of Westmoreland, Allegheny, Beaver, Lawrence, Butler, and Armstrong Counties were consulted. Basic data about the "Depreciation Lands" were found in a brief but authoritative article in the *Pennsylvania Archives*, series 3, Vol. 3: 761–771, and on its accompanying map. The microfilm of the extant issues of the *Pennsylvania Gazette*, 1780–1787, was also examined, but no mention of Franklin's tract was there found.

Exploratory trips were necessary. Since the "Depreciation Lands" were until 1788 in Westmoreland County, an early trip to the courthouse at Greensburg was made, but no documentation of Franklin's tract could be located. Trips to the Allegheny County Courthouse in Pittsburgh likewise failed to reveal any documentation of the matter though the tract lay in Allegheny County from 1788 to 1800. This lack of documentation is not surprising, for the "Depreciation Lands" were an affair of the Commonwealth of Pennsylvania and not a county matter, save, possibly, as to taxation; and early county tax records are commonly not extant.

In research in county records, a greatly important fact, understood mainly by title lawyers and genealogists (but sometimes not by historians) is that where early records are not extant (either never entered or later destroyed) much information, including many old documents, is sometimes to be found in much later records of deeds, wills, and litigation. Such proved to be the case about Franklin's "lands near the Ohio."

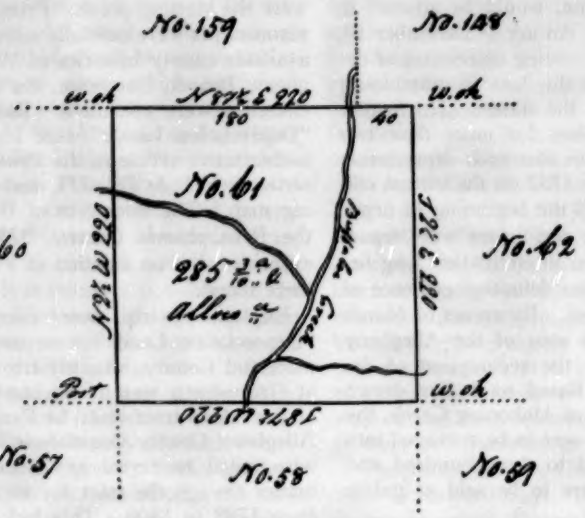
On the first trip made to the Armstrong County Courthouse, at Kittanning, there was found, in the Recorder's Office, Deed Book No. 3, folios 16–18, a deed of October 29, 1814, from "Louis Bache & al to Randall Hutchinson." In this deed, as recorded, appeared not only the basic facts about Franklin's Ohio Valley lands, but also numerous clues to additional material at Kittanning, Butler and Harrisburg.

Data in the above deed showed (1) that Benjamin Franklin at the time of his death was in possession of eleven parcels of land, Lots 61–65 and 81–86 in the Joshua Elder Survey of District No. 5, of the "Depreciation Lands"; (2) that he devised the entire tract to Richard Bache, his son-in-law, who kept it intact during his lifetime; (3) that Richard Bache, in turn devised it as part of his estate to be equally divided among the seven

[11] Not found, but implied in the law of March 1, 1780, *Laws of the Commonwealth of Pennsylvania* by M. Carey and J. Bioren, 239, Philadelphia, 1803.

[12] *Ibid.*, 275, but by title and footnote only. Printed in full in *Statutes at large of Pennsylvania from 1682 to 1801*, compiled by J. F. Mitchell and Henry Flanders, **10**: 233–238, Harrisburg, 1896–1908.

[13] *Ibid.*, 398–401. The laws of 1783 were strangely omitted by Mitchell and Flanders.



Survey'd on the 3d day of June 1785 the above described Lot of ground situate on the west side the Allegany River on the waters of Buffaloe Creek containing Two Hundred & eighty five Acres & three tenths of an Acre & Allowance of six P. Cent. & C.a By Virtue of a law passed the 12th day of March 1783 intitled an Act for the sale of certain Lands for the purpose of Redeeming & paying off the Certificates of Depreciation given to the Officers & soldiers of the Penn-sylvania line &c

Joshua Elder D.S.

John Lukens Esq.r S.G.en.l

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FIG. 3. Survey of Lot No. 61. Courtesy of Pennsylvania Land Office.

families of his seven children; (4) that Richard Bache's executors and heirs quickly took measures to partition into seven parts the lands of the tract; and (5) that accordingly the eleven lots were deeded to Randall Hutchinson that he might make out the respective seven deeds assigning to each group of heirs its respective lot or lots.

In the deed to Randall Hutchinson and in the seven distribution deeds, recorded later at varying dates, were found precise references to the location, in the Pennsylvania Land Office at Harrisburg, of the records of the eleven individual surveys and of the eleven individual patents.

The first of several visits to the Land Office in Harrisburg fully confirmed the basic date of the eight deeds of October 29, 1814, and furnished additional material containing information not available at Kittanning.

An inventory volume entitled, "Depreciation Lands," listed on pages 47–48, the numbers of the lots, the purchaser of each, the acreage of each, the date of the patent of each lot, the final patentee, and the exact location of the patent record and survey of each lot. The same volume, page 68, mentions "Depreciation Certificates Patented"; District 5; the lot numbers: "Franklin Benj'n" (as patentee); and a cross reference to pages 47–48. And on pages 102–103 are found the depreciation ticket numbers, 22–32; the purchaser, "Benjamin Franklin Esq"; the numbers of the eleven lots; the "orders to accept"; the dates of the warrants; and the exact location of the eleven patent records. According to pages 102–103, the date of each of the eleven warrants was September 23, 1787, and according to pages 47–48, the date of patents for Lots 61 and 62 was October 8, 1787, for Lots 63, 64, 81, 82, 83, 84, and 85, was October 10, 1787, and for Lots 65 and 86, was October 11, 1787, data not found elsewhere.[14]

On file and in a state of excellent preservation in the Land Office is the original regional or District No. 5 Survey of Joshua Elder, on which Lots 61–65 and 81–86 are clearly indicated. A photostat of the desired part of this complete Joshua Elder Survey was secured.[15] On file also are the original eleven individual lot surveys, of which photostats were secured.[16] Available also were the eleven official records of the patents, the originals having been retained as usual by the patentee and handed down to later heirs and possessors. Photostatic copies of official record copies of the eleven patents were secured and later submitted to the Society.

From the twelve surveys and the eleven patents appear adequate data about the location of Franklin's "lands near the Ohio." Ten of the eleven lots lay in what is now South Buffalo Township, Armstrong County, Pennsylvania. Lot No. 61 was so largely in Butler County that its later official records are in Butler and not in Kittanning.

The nature or character of the "Tract" is clearly established. Franklin's eleven lots, 61–65 and 81–86, but for the non-possession of Lot 80, included a stretch of land about four miles east to west and seven-tenths of a mile north to south. The Tract extended across South Buffalo Township from the Allegheny River to and beyond the western boundary of Armstrong County.[17] Eight of the eleven lots were inland on the rolling hills and valleys of a high plateau, today an extremely rich dairying area. Three of the lots were riverside territory, then, and later, rough, hilly and subject to erosion. In size (though not in shape) Franklin's "Tract on the Ohio" was the equivalent of four sections of a township such as provided for in the contemporary Survey Ordinance of 1785. In common phraseology, Benjamin Franklin during his last years was in the possession of eleven or more good farms in western Pennsylvania.

The series of actions by which between 1781 and 1788 Benjamin Franklin came into possession of his Ohio Valley lands is not established by documentation, which is almost entirely lacking for the years 1781–1784, and both inadequate and confused for the later period. Implication and imagination, both of them dangerous historical methodology, are needed to fill in the gaps.

The first material step was the legislative act of December 18, 1780 which made provision for issuing depreciation certificates, which conceivably may have been issued, received by individuals, and then sold or transferred to others, from 1781 to 1787. Richard Bache either for himself or for Franklin may have secured early depreciation certificates as also John Nicholson may have done. According to the "Inventory of the Depreciation Lands," pages 102–103, Benjamin Franklin presented depreciation certificates Nos. 22 to 32.

[14] Merely for convenience, collated typescripts of the pertinent data of the inventory volume have been filed with the Society.

[15] The photostat was filed with the Society.

[16] Similarly filed.

[17] The photostat of the part of Elder's complete survey can easily be superimposed upon a map of South Buffalo Township.

The act of March 1783 designated the lands to be auctioned off in order to liquidate the depreciation certificates. The surveys of the "Depreciation Lands" began in early May 1785.[18] Between June 3 and June 10, Joshua Elder surveyed the eleven lots later acquired by Benjamin Franklin.[19] The last individual lot survey by Elder was made October 28, 1785,[20] after which he probably returned to his home in Lancaster and later to Philadelphia. As a rule, in the eighteenth century, considerable time elapsed between field surveys and the return of certificates of survey to the land office. The eleven individual surveys under consideration are endorsed as returned October 8–11, 1787. But in the interim between the surveying and the return of the certificate, Franklin, in April 1786, wrote, "I have a large Tract on the Ohio."

Owing to failure to find preserved either in Harrisburg or in Philadelphia any depreciation certificates, auction sale sheets or similar vouchers, the exact date at which Lots 61–65 and 81–86, in the Joshua Elder Survey were sold at auction could not be determined. According to the article in the *Pennsylvania Archives*, series 3, Vol. 3: 761–771, though lots in other districts of the depreciation lands were sold earlier, "The . . . sales were . . . held in Philadelphia, where between the sixth and twenty-seventh days of September 1786, 336 tracts in Joshua Elder's district No. 5, containing 82,337 acres were sold at prices that varied from seventeen shillings six pence down to three pence per acre." In the absence of vouchers and in view of Franklin's claim of April 1786, and the attested fact that the certificates of survey were not returned until October 8–11, 1787, discrepancy is obvious and confusion unavoidable. The answer may well be, however, that the facts of the Joshua Elder surveys were well known as early as the winter of 1785–1786 and that it was common in the eighteenth century to sell land grants duly surveyed, long before the return of the certificates of survey to the land office.

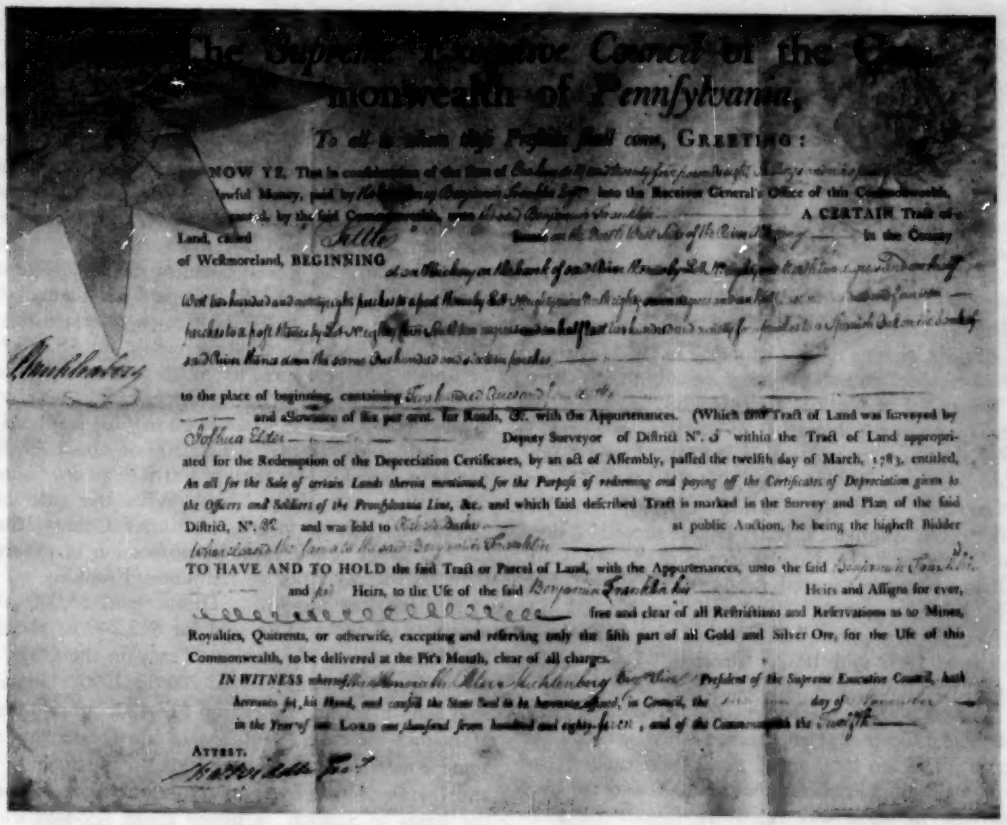
It has been observed above that according to the inventory volume of the "Depreciation Lands," pages 102–103, the date of the eleven warrants is given as September 23, 1787 and, on pages 47–48, the date of the patents as October 8–11, 1787, the second date probably involving a confusion of certificates of survey and patents, for the surveys are clearly marked "Ret^{d} & C"., October 8–11, 1787.

The eleven patents were all signed and sealed, "the first day of November 1787." Five of them were enrolled November 30, 1787, and the other six so enrolled December 3, 1787. Ten of the lots, according to the patents, were "sold to Richard Bache at public Auction, he being the highest bidder." Lot No. 83, with a lateral length along the Allegheny River, was similarly sold to John Nicholson. All eleven lots must have been released to Benjamin Franklin before November 1, 1787. When they were sold to Bache and Nicholson and when released to Franklin will probably never be known. But somehow Franklin in his last years got possession of a valuable tract of land in western Pennsylvania.

The financial aspects of the acquisition of the eleven lots "near the Ohio" are a matter of historical and biographical interest. What Bache and Nicholson paid at the auction sales is not known. Probably the amount for each lot corresponded closely with the sum paid for each lot by Franklin. According to data in the patents, Franklin paid for the eight inland lots, a total of £136—7–3, roughly thirty-two cents per acre, and for the three riverside lots, £555—5–0, or about $1.00 per acre, a price more than double the average price per acre of the entire Depreciation Lands. While he paid little more than half the average price for the eight inland lots he paid six times the average price for the riverside lots.

As early as, and probably before, November 1, 1787 each of the eleven lots in Franklin's "Tract" had been given a name,[21] but by whom is not certain. They may, theoretically, have been given by Joshua Elder the surveyor, for in the seventeenth century names for a tract frequently appear in the original survey, and the Land Office may have named each survey. But it is highly probable that the names were given by Richard Bache, for such names as "Preston," "Kirkham," "Walton," "Poulton," "Wigan," "Bolton," "Garstang," "Clifton," "Broughton," and "Chorley" smack of old England and especially of Lancashire, while "Settle" was the western repetition of "Settle" the native place of Richard Bache in Yorkshire, England, and also the name of his homestead estate in Berks County, Pennsylvania. The possibility that Franklin, long resident in England, as-

[18] *Penna. Arch.*, ser. 3, 3: 761–771.
[19] Data from the surveys as filed.
[20] Data from the survey of Lot No. 314.
[21] The names are on the patents.



The Supreme Executive Council of the Commonwealth of Pennsylvania,

To all to whom these Presents shall come, GREETING:

NOW YE, That in consideration of the Sum of [illegible] lawful Money, paid by [illegible] into the Receiver General's Office of this Commonwealth, [illegible] by the said Commonwealth, unto the said [illegible] A CERTAIN Tract of Land, called Settle [illegible] in the County of Westmoreland, BEGINNING [illegible] and allowance of Six per cent. for Roads, &c. with the Appurtenances. (Which said Tract of Land was surveyed by Joshua Elder [illegible] Deputy Surveyor of District N°. 3 within the Tract of Land appropriated for the Redemption of the Depreciation Certificates, by an act of Assembly, passed the twelfth day of March, 1783, entitled, *An act for the Sale of certain Lands therein mentioned, for the Purpose of redeeming and paying off the Certificates of Depreciation given to the Officers and Soldiers of the Pennsylvania Line, &c.* and which said described Tract is marked in the Survey and Plan of the said District, N°. 82 and was sold to [illegible] at public Auction, he being the highest Bidder [illegible] TO HAVE AND TO HOLD the said Tract or Parcel of Land, with the Appurtenances, unto the said [illegible] and [illegible] Heirs, to the Use of the said [illegible] Heirs and Assigns for ever, free and clear of all Restrictions and Reservations as to Mines, Royalties, Quitrents, or otherwise, excepting and reserving only the fifth part of all Gold and Silver Ore, for the Use of this Commonwealth, to be delivered at the Pit's Mouth, clear of all charges.

IN WITNESS [illegible] President of the Supreme Executive Council, hath hereunto [illegible] and caused the State Seal to be hereunto affixed, in Council, the [illegible] day of [illegible] in the Year of our LORD one thousand seven hundred and eighty-seven, and of the Commonwealth the [illegible]

ATTEST.

FIG. 4. Patent of Lot No. 82, "Settle." Original owned by the American Philosophical Society.

signed the names on taking out the patents cannot be ignored, but it is very remote.

What use, if any, Franklin made of his eleven lots before his death could not be determined. They were wilderness territory and all in the category of "unseated lands." If taxes were levied upon them by Allegheny County, of which no evidence could be found, they must have been duly paid, for on later transfers of title no tax liens appear. Very generally old tax records are not well preserved and sometimes they are, with legal governmental authorization, deliberately destroyed.

On the death of Benjamin Franklin in 1790, the eleven lots came into the possession of Richard Bache who kept the property intact until his death on July 29, 1811. From 1790 to 1800 they lay in Deer Township, Allegheny County, and thereafter, with the exception of Lot No. 61 (mainly in Butler County) they were in South Buffalo Township, Armstrong County. No fiscal papers of the two decades has survived at Pittsburgh or at Kittanning.

But no liens on the property came up in 1814 or later. And in the absence of records it is not known whether the eleven lots were carried on the county books as "Unseated Lands," a common designation of the time and region, or whether they were occupied by tenants. Farm leases, then, and to this day, and other such items are not usually a matter of public record. The same lack of information about use made of the property features the period from July 29, 1811 to October 29, 1814 when the eleven lots were controlled by the executors of Richard Bache. It was however, during this period of three years that the heirs of Richard Bache, "agreed upon a partition and division of the above mentioned Eleven Tracts of Land in Order that each of the said parties . . ."

should ". . . have and hold one seventh part thereof—in value—in severalty." [22]

This partition and division agreement was carried out in the deed to Randall Hutchinson and the contemporaneous seven deeds by Hutchinson to the heirs. A noticeable aspect of this highly detailed matter is that each of the three water front lots was considered a full seventh share, while two inland lots, invariably contiguous, were required in the remaining four shares.

The ink of the signatures of the deeds of October 29, 1814, may not have been dry when William J. Duane began the process of buying the other six shares. On October 21, 1814, Richard Bache, Jr., and his wife deeded their share, "Wigan," Lot No. 65, and "Bolton," Lot No. 84, to Duane in consideration of the sum of $1,600.[23] For the same consideration, $1,600, Louis Bache and his wife deeded, July 27, 1818, to Duane, "Garstang," Lot No. 85, and "Clifton," Lot No. 86.[24] On August 13, 1827, Duane secured a deed to "Chorley," Lot No. 83, for $500.[25] In 1829, Duane similary secured deeds to "Walton," Lot. No. 63, along with "Poulton," Lot No. 64, for $500;[26] "Broughton," Lot No. 81, for $400,[27] and between 1829 and 1832, "Preston," Lot No. 61, and "Kirkham," Lot No. 62, for $400.[28]

For $5,000 Duane bought the six other shares, about 2,337 acres at an average price of $2.14 per acre. The ten lots, a full generation earlier, had cost Benjamin Franklin about $2,000 and the total increase in value was relatively small. In addition to the ten lots bought, Duane and his wife were by inheritance in possession of "Settle," Lot No. 82, worth probably $500 in 1831.

There is no evidence that Richard Bache or any of his heirs thought of pioneering on the "lands near the Ohio." William J. Duane and his wife, in possession of 2,500 acres of land worth $5,500, continued his Philadelphia residence. With large eastern interests and connections Duane had no wish to become a wilderness magnate of western Pennsylvania. Facts make it obvious, also, that he did not wish to be an absentee landlord. He may possibly have bought the other six shares on the solicitation of the less well-to-do relatives of his wife, and it is a reasonable conjecture, that as a good business man, he welcomed the prospect of some profit on his investment. Before he had completed his purchases, Duane began to sell some of the lots. Between April 13, 1829, the date of the first sale,[29] and December 11, 1833[30] he sold eight of the eleven lots for $7,940, a neat profit of $3,540 on the $4,000 paid for them. A decade later, January 24, 1840, two riverside lots, "Broughton" and "Settle" were sold for $8,180[31] or about $20 per acre, as much as some of the acreage was worth a full century later. Finally, April 1, 1842, Duane sold "Preston," his last holding of the old "Tract," for $1,300 or about $4.55 per acre, a price less than a thirtieth of its value more than a century later. With the sale of "Preston," the lot largely in Butler County, the entire "Tract" came into the possession of others than the descendants of Benjamin Franklin.

The ten lots for which Duane paid $5,000 or about $2.14 per acre he sold for $13,240 or about $5.63 per acre. The entire "Tract on the Ohio," for which Franklin paid $2,700 in 1787, Duane sold half a century later for $17,420, more than a six-fold increase. But late in life Duane made some concessions and remissions on mortgages[32] and thereby reduced the total amount received for the lands to $16,737, a figure about six-fold the amount originally invested by Franklin.

In the management and disposal of the eleven lots Duane was neither precipitate nor negligent. In some cases he probably sold to former tenants. There are indications that he made trips to Kittanning in May 1829, May 1830, June 1832, May 1842, and, probably, May 1845. Of the records found, two only, both "amicable actions," were

[22] Citation from the deed of the executors to Randall Hutchinson, October 29, 1814.

[23] Armstrong County, Pennsylvania, Deed Book No. 3, folio 21, recorded March 16, 1820, more than five years later.

[24] *Ibid.*, folio 523. Likewise recorded, March 16, 1820.

[25] *Ibid.*, Deed Book No. 7, folios 496–498. Recorded June 16, 1832.

[26] *Ibid.*, folios 507–508.

[27] *Ibid.*, folios 500–501.

[28] *Ibid.*, folios 494–496. Three deeds, all recorded, June 16, 1832.

[29] *Ibid.*, Deed Book No. 6, folios 307–308. Recorded July 24, 1789. Sold to Andrew Shrader, the original deed in recent years in the possession of Miss Emma Grader, a granddaughter, R.F.D. 1, Freeport, Pennsylvania, who, vigorous and gracious, lived on a part of old "Wigan."

[30] The deeds are all recorded in the Deed Books. Collated typescripts of them are filed with the Society. Photostatic service, later installed, makes it possible for a small sum to secure reproductions of any one or, indeed, of all of the documents.

[31] *Ibid.*, Deed Book No. 12, folios 33–36.

[32] Collated copies of these are filed with the Society. One of them, an unrecorded power-of-attorney, is in the handwriting of Duane.

judicial, for Duane was "resolutely opposed to litigation." [33] The satisfaction of an old mortgage, March 21, 1851, was the final act by Duane.

The legal and financial and some of the social history of the "Tract" from 1814 is unusually well documented in the official records of Armstrong and Butler Counties. Directly connected with one or more of the heirs of Richard Bache are thirty deeds, five powers-of-attorney, four mortgages, two quit claims, and two amicable suits. Transcribed and collated copies of these documents have been filed with the Society.

Well preserved and easily accessible in the official records of the two counties are hundreds of documents [34] related to the lands from the time of the sale by Duane down to the present. Where Franklin, Bache, and Duane had eleven titles there are today, as a result of much subdivision of farms, and many surburban and summer homes, scores of titles. But very few of the present owners or occupants of the lands of the old "Tract," are aware that they were first privately owned by the celebrated Benjamin Franklin.

[33] Statement of biographer, in *D. A. B.*

[34] County histories are frequently of little value. An exception is *The history of Armstrong County*, Pennsylvania by Robert Walter Smith Esq., Chicago, Waterman, Watkins and Co., 1882, a scholarly but rare volume. Smith carefully examined the Armstrong County items, commenting on those herein cited and adding some comments about items not directly related to Franklin, Bache, or Duane.

REGIONAL COOPERATION IN EDUCATION

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(Read April 23, 1954)

I WISH to speak of an experiment, now in its sixth year, in regional cooperation for professional and graduate education resulting from an interstate compact involving fourteen southern and southwestern states stretching from Maryland to Texas. Interstate compacts are not new to this country but in the past these usually have involved some facility of common benefit to citizens of two or more neighboring states, such as a bridge, and did not require a continuing administration. In the case of the Southern Regional Compact, however, an attempt is being made to implement and carry out a complicated and long range cooperative program of professional and graduate training and is, thus, of some unusual interest. For the extent to which regional needs can be dealt with successfully by compacts between the states, the necessity of calling in the federal government can be avoided. Furthermore, in view of the vast hordes of students which are now crowding the high schools over the country, and who will soon be demanding an opportunity for professional and technical graduate training, it may well be that certain devices now being developed under the Southern Regional Pact may have widespread applicability in other regions, in the years immediately ahead.

In 1947 the governors of fourteen states signed a Compact involving cooperation in the use of existing facilities in the several states for professional and graduate training. This Pact, subsequently ratified by the several legislatures of the states concerned, created a Board of Control for Southern Regional Education to implement and carry out the objectives of the agreement.

At the time this Pact was signed, it was widely misinterpreted, in the public press, as an attempt to solve the problem of Negro professional education, and while it is true that Negroes do benefit in certain states from this agreement, the governors had a much broader educational problem in view. With a shift from a one crop economy to diversified farming, and especially with the industrialization of the South and Southwest during and following World War II, which greatly improved economic conditions throughout this region, many educational needs surged into the foreground, such as training research workers to man industrial research laboratories, and the governors, some of whom earlier had been trustees of large universities, were mindful of the inadequacy or, indeed, the total absence in some of the states of facilities for training in certain professional and graduate fields. Because of this, for generations some of the ablest young people of the South had been forced to go elsewhere for advanced training and, too often, did not return to enrich the intellectual life of their home institutions and communities. At the same time, the governors were equally aware of the pressing and ever increasing demands for undergraduate training in tax supported schools and of the very high costs of establishing and maintaining professional schools which would serve the needs of relatively few students.

From these considerations there emerged the idea of making available to the "have not" states the facilities already existing in other states of the region upon the payment of suitable compensation. Veterinary medicine will serve as an illustration. There were already five veterinary medicine schools in the region which were judged adequate to serve the needs of the fourteen states. If each state were to establish such a school of its own, it would have called for an outlay of many millions of dollars for physical plants, and equipment and very high maintenance costs because of the low number of students wishing such training. Under the Southern Regional Pact, as it has been worked out, the "have not" states, in effect, lease the veterinary teaching facilities now existing in other states and pay $1,000 per student per year, which approaches the actual cost of instruction. In this way, the budgets of these schools are measurably increased and since all are operating at more nearly full capacity, the per student cost is reduced.

There is a similar arrangement for human medicine and for dentistry, involving the payment of $1,500 per student and for training in social work at a cost of $750. The mechanics of operation of these so-called Service Contracts is simple. Each state decides, through appropriate agencies, how many students it wishes to provide for in the several areas of training and then its Legislature appropriates to the Southern Regional Board (actually, or in effect) the funds needed for such service and for its share of maintaining the Central Office. The personnel, in the latter office make contracts with either public or private schools of the region for the service needed and disperses funds appropriated for this purpose. At present ten tax supported and ten privately endowed schools benefit under such contracts. All students must meet entrance and scholastic standards of the school they enter and must be acceptable otherwise to that institution.

This Contract Service has been in operation for five years and currently 1,069 students are crossing state lines for training in the four fields mentioned. This phase of the experiment has been so successful and satisfactory that there has been established a Western Interstate Commission for Higher Education which is working out a similar interstate cooperative program and it has been reported that some New England states are giving consideration to a similar solution of some of their educational programs.

Now a word about how this interstate agreement is administered. The Southern Regional Education Board consists of the governor and three educational representatives appointed by him, or his predecessor, of each state. The educators are usually the presidents of the state university and of the Negro land-grant college and the state superintendent of education. The whole Board meets annually but most of its business is transacted by an Executive Committee composed of four governors and eight educators. This committee meets as frequently as needed, usually for two days, three or four times a year. At such meetings the Executive Director of the Central Office submits for consideration and approval any new projects to be undertaken.

The operating agency of the Board is a central office in charge of an Executive Director and much of the success of this undertaking to date rests on the initiative, vision and common sense of the Director and his staff. For the most part, broader regional problems are considered by commissions, or special committees, which are representative of both public and private institutions of the whole region and to which are added as consultants usually two individuals from other parts of the country. Thus there is an opportunity for a cross-fertilization of ideas. Very wisely, abundant funds are set aside by the Board for regional meetings and it may well be that the most valuable contribution made by the Pact will be benefits which accrue from such gatherings when educators with diverse backgrounds sit down and discuss regional needs. In addition to administering the various programs and acting as the fiscal agency of the Board, the personnel of the Central Office serve as an overall planning and research group, and as a clearing house of information for the use of the colleges of the region. As needed, from time to time, additional specialists are added temporarily to the staff to assist in making surveys or special studies, approved by the Executive Committee, and many very useful bulletins have resulted. For example, there has recently been completed a careful study of past trends in college enrollments of the region and on the basis of this a prediction has been made of the probable future enrollments which the colleges of each state may reasonably expect during the next fifteen years. This information has been placed on the desks of the governors and educational agencies of the several states so that those concerned with public education, at the college level, can plan more intelligently for the years ahead.

The Central Office is supported by a membership fee paid by each state and by numerous grants from foundations, which see in the regional approach, an opportunity to reach very large numbers of institutions.

One of the original objectives of the Pact was to make available to the students of the region first class graduate instruction and as soon as the Contract Service program was well launched, the effort of the Board, working through the Central Office and the various regional committees, has centered on ways and means of improving graduate education, more especially at the post-Master level.

An inventory of graduate facilities of the region was first made and this revealed that there was an adequate number of graduate schools already existing and all important fields were represented. It was obvious that what was needed was not more

graduate schools but better ones and, in the last analysis, this becomes a matter of the recruitment to the colleges and universities of the region of more well-trained and productive scholars. It is true, of course, that successful graduate teaching involves many things such as library, or special equipment, but if competent teaching personnel is lacking the other factors are of little avail. Since the recruitment of staff and adequate research equipment are local campus problems, the efforts of the Board must be indirect in character. You may be interested in two of the several ways the Board has set about assisting in the solution of this problem.

An all too common failing of educational institutions is to offer graduate training in too many fields thus spreading often meager funds very thin and resulting inevitably in mediocre or poor training. The southern region can claim no special distinction in this regard! To counter this trend, the regional Board has sponsored so-called Memoranda of Agreements between institutions offering graduate work in subjects of limited demand such as forestry, the petroleum sciences, city planning and the like. The basic idea back of this development is this. In highly specialized fields, such as those mentioned, it is scarcely possible for one institution to provide adequate personnel and the expensive and specialized equipment to cover research training in the whole, for example, petroleum field. It would be much better if a single institution would concentrate on one aspect—such as the secondary recovery of oil, or oil-gas phase relations, or refining, and offer outstandingly good training, in just one area, initially at least. The Memoranda of Agreements simply indicate the willingness of the administrations of the several schools offering petroleum graduate work, to sit down and study the regional demand for such training and plan collectively, to the end that there may be in the region at least one institution with first class research equipment and teaching personnel for training for a particular phase of petroleum research. Once this is accompiished any school is free to develop other fields as they may wish.

It is much too early to predict how effective these Memoranda will prove to be. It takes time to recruit an outstanding graduate faculty and requires a great deal of money for essential equipment. On the other hand, some sixty-four institutions of the region, well knowing the purpose of the Memoranda, have signified their willingness to cooperate in joint planning and details of many such agreements are now being worked out. Those of us who have been assisting these efforts are optimistic about the ultimate value of this endeavor in view of the tremendous undergraduate student pressure which will reach a peak about 1970, which will strain the financial structure of all schools, and we feel that these Memoranda will point the way for the solution of graduate education in many fields more commonly in demand.

The Southern Regional Board is primarily concerned with the tax-supported institutions and most of the financial support from the legislatures of the respective states. The better these representatives are informed about educational needs, the more wisely they can supply the money needed. To aid in this the Board sponsors annual Legislative Work Conferences. Each governor appoints three or four of the most influential legislators of his state to attend, and for two days these representatives of the people are given exact and objective information about the present status of professional and graduate education of the region, and incidentally see their own state in a regional setting, they are informed about overall regional needs, of the wisdom of cooperative planning between institutions, and most important of all, what is necessary to make institutions of higher education really first class in a national sense.

Two such conferences have been held and they have been very enthusiastically received by the legislators who have attended, because, as they say, for the first time they have obtained unbiased information and seen the problems in a wider perspective. After all, most men want to do what is in the best interests of the people of their state, and when they clearly understand the situation it is reasonable to expect effective action at the state level.

These Legislative Conferences have been so successful that this Fall the Board is sponsoring a similar conference of the administrative heads of state schools. It is felt that they too may benefit from an overall regional perspective.

After seven years of intimate association with the planning and the activities of the Southern Regional Board, I am very optimistic about the effectiveness of this cooperative regional effort. But I would not be frank if I did not tell you that some Southern educators have been very skeptical or even critical of the Southern Regional Board.

Most of this reaction is due, I think, to a lack of clear understanding of the objectives of the Board. Some say, and they have a point, that no politically dominated board can properly deal with educational problems. But this Board, though appointed in part by the governors, is largely composed of the leading educators of public institutions of the region, and I think we have nothing to fear if these men will give their best efforts in the future as they have in the past, to the cause of education in the South. In any event, when a facility is provided by which governors, White and Negro educators sit down frequently in a frank, friendly atmosphere to discuss the problems of what is perhaps the most vital activity of our society, higher education, a sound basis is established for the solution of many other problems besides professional and graduate instruction.

CELESTIAL NAVIGATION

PHILIP VAN HORN WEEMS*

Captain, United States Navy (ret.)

(*Read April 23, 1954*)

SINCE the science of celestial navigation has had more than two centuries of development involving astronomy, mechanics, physics, and mathematics, no attempt will be made to cover the entire subject. Rather, I shall describe briefly its practical beginnings about 1734, its status in 1800, in 1900, and at present, and then indicate some possible future developments.

The urgent need for better navigation mothered twins—the sextant and the chronometer—about 1734. This advance was so great and the need so fully met that by 1800 celestial navigation methods became standardized. Relatively few changes were made in the nineteenth century, though special credit is due Sumner and St. Hilaire for their valuable contributions. Celestial navigation at sea may be fairly dated from the inventions, at nearly the same time, of the sextant and of the chronometer.

Since the only practical means for determining longitude requires an accurate timing device, there was no means for determining longitude at sea until Harrison in 1735 built his chronometer No. 1, which is still in running condition at Greenwich, England.

The sextant was invented nearly simultaneously, and independently, by Thomas Godfrey of Philadelphia and John Hadley of England about 1733. This was such a great advance over the old cross-staff that it is believed that even an authentic model of the latter does not exist today.

To supplement these two observing devices, appropriate tables and a better nautical almanac were needed. By 1802, Bowditch supplied excellent nautical tables, with the result that celestial navigation became standardized and sufficiently accurate to meet the needs of the mariner. The practice of navigation in 1900 was not greatly different from that of 1800. The same vernier type sextant was in use with moderate improvements; Percy L. H. Davis had his improved haversine tables, and there was a clearer understanding of the celestial line of position. The method of timing observations was little changed. The almanac and the nautical tables in 1900 still catered to navigators using lunar distances, though I have never known a navigator who knew a person who made practical use of lunar distances.

Since 1900, we have had radio communications, but the navigators in general stuck to their elaborate chronometer comparisons, with their first and second differences and complicated chronometer corrections.

The turn of this century saw in use on larger ships: (1) the vernier sextant; (2) three chronometers, carefully rated and compared, with time checks by the "time ball" in port; (3) the American Nautical Almanac, using "Astronomical Time," and including data for working "lunars"; (4) American Practical Navigator, by Bowditch with some of the tables a century old.

Soon air navigation supplied the need, and the radio the means, for radical improvements in celestial navigation. As marine navigation methods were streamlined for air navigation, the innovations were later adopted by mariners. Some of these developments include the following matters:

1. Timing of observations has been improved by the use of radio time signals and the second-setting watch, giving Greenwich mean time by direct reading. The three chronometers formerly used have been reduced to one, and this is used normally as a check only, since the navigation watch is checked and rated by radio time signals, and set to the exact second of Greenwich mean time. The *timing* of observations is one problem which has been satisfactorily solved. If the sextant observation for altitude could be made as ac-

* On November 13, 1953, the Society in Executive Session voted to award the Magellanic Premium to Captain Weems in recognition of his invention of methods and instruments for celestial navigation. The Magellanic Medal was presented to him on April 23, 1954.

curately as we can time it, the observed data would then be nearly perfect, say to within one hundred feet.

2. The vernier of the old marine sextant has gradually given way to the *micrometer drum*, and the bubble sextant with averaging device has been developed for air navigation.
3. Special tables have been published which cater to the needs of the navigator. These tables speed up and greatly simplify the process of reducing observation to position.
4. The nautical almanac has undergone radical changes by incorporating the features of the air almanac.

Since time does not permit an account of each development, I shall discuss the air almanac. Partly by accident and partly by inclination I have contributed personally to its modern development.

In 1928 at Charles A. Lindbergh's request, I had a month's leave from the Navy in order to teach him celestial navigation. I reported to Major Tom Lanphier at Selfridge Field in Michigan. While working sun lines of position for practice, Lanphier pointed out the weakness of advancing a sun line for approximately an hour's run to cross with a second line to obtain a running fix. We in the Navy had, of course, known about this weakness of celestial navigation, but it hadn't occurred to us to do anything about it. Lanphier's observation was a challenge, and, as a result, I began to study the problem. A few days later in a Washington hotel, I awoke about four A.M. from an apparent dream with the idea, "Why not simplify moon sights by tabulating the moon's right ascension and declination against sidereal instead of mean time in order to save the conversion of mean to sidereal time?" To make certain I would not forget the idea, I wrote it down immediately. However, this was unnecessary since it was on my mind when I awoke about seven o'clock.

The idea still seemed sound to me, but it would be ludicrous to go to the hydrographer with the story, "I woke up last night with an idea." I compromised by saying as casually as I could, "Recently I had an idea of speeding up moon sights." To my pleasant surprise the hydrographer saw nothing wrong with the idea. I then wrote an official report on the subject, and later went to the Naval Observatory where the superintendent gave me every encouragement. I wrote a second report on the subject and started work on producing the new moon data.

Several friends, the late Rear Admiral I. C. Sowell, Vice Admiral M. F. Schoeffel, Vice Admiral J. E. Ginrich, and others offered helpful suggestions such as giving right ascension in *arc*. I wrote Dr. Ogura in Japan about our problems. He answered, "I do not see why you do not use Greenwich mean time in the first place."

So it went, one faltering step at a time. Before Dr. Ogura's letter was received, the practical solution came to me while I was pacing the deck of the U.S.S. *Cuyama:* "For each selected interval of Greenwich mean time, compute and tabulate the moon's Greenwich hour angle in *arc*." I made official reports after each improvement. Finally in September, 1929, appeared "The Lunar Ephemeris for Aviators" giving the moon's Greenwich hour angle and declination for ten minute intervals of Greenwich mean time. Later this plan was extended to the sun, planets and stars. The first "Air Almanac" of my design appeared in 1933, and was discontinued in 1934. In 1936 the idea was suggested to the British, who published it in 1937. I then published it under license in the United States. Finally in 1941 the United States started regular publication of the "Air Almanac." Now the British and U. S. publish it jointly.

VISION OF THE FUTURE

Billions have been spent and thousands of inventions have been made of electronic equipment within the past fifteen years. As a result of this tremendous effort, great progress has been made. Even so, because of the fact that electronics methods may be jammed, lack of availability, cost and weight, and lack of accuracy under certain conditions, electronic methods do not yet fully meet the requirements for sea and air navigation.

Progress in celestial navigation has slowed in recent years and much-needed research and tests in this field would probably pay good dividends for effort expended. Celestial navigation suffers from age. It has sufficed for the mariner for so many years that many seem to think that it is a fully developed art and needs no further development.

The following items should undergo research and careful tests: (1) refraction and irradiation; (2) deflection of the vertical; (3) coriolis effect; (4) acceleration.

Refraction tables, especially for low altitudes, are to some extent uncertain. An urgent need exists for systematic, controlled tests to determine

values of refraction under various conditions of temperature, pressure, and especially of terrestrial refraction which appears in the dip of the horizon. Perhaps this Society could help in this important work.

As research and tests improve the art of celestial navigation, its application should be greatly improved by combining electronic controls to celestial observations. After all, electronics, per se, do not suffice for our purposes. A selenium cell will control the opening and closing of a door, but we need the door and the power to operate it.

If we have a precise gyro, electronic methods for controlling it will do wonders, but we must first have the gyro. In the earth itself we have a most accurate gyro. Furthermore we know the positions of numerous heavenly bodies so accurately that when these positions are translated to the earth, we can fix our position to within an area the size of this room. We survey our lands with great precision by establishing fundamental positions by celestial observations. Once a position is accurately established, we can do wonders with electronic and other methods in determining *relative* positions.

Navigation in the future would benefit by utilizing the positions of heavenly bodies so accurately determined by astronomers together with the art of the practical navigator aided by electronic methods of control.

PUBLISHING HISTORICAL SOURCES

A Prejudiced View of the Problem of Finance

CHESTER KERR

Secretary, Yale University Press

(Read November 12, 1953, in the Symposium on the Manuscript Sources of American History: Problems of their Control, Use, and Publication)

As originally approved, this paper was entitled "Publishing Historical Sources: Problems of Design, Production, Finance, and Sale." Then it seemed desirable to limit that formidable subtitle to read, "A Prejudiced View of the Problem of Finance." Finance, after all, is the most imposing problem of all. The questions of design, production, and sale no doubt need canvassing but may be set aside here in favor of that chronic uncertainty once described by David H. Stevens as "how and from what source may come the essential funds for long and wise plans." [1]

The view of this problem taken here is the prejudiced one of an officer of an American university press, which is different from that of a commercial publisher, and it is also the view of a scholarly publisher of books, rather than periodicals. Again this is not to say that the question of publishing historical sources in scholarly journals does not need review, or that the publications problems of commercial houses and the learned societies should be overlooked, but that they will not receive their due in this brief paper. It will deal, instead, with this vexing problem of finance as it affects the publication of historical sources in book form by university presses.

No doubt financing the publishing of such sources has always been a problem, even though Mr. Butterfield suggests this was less so a hundred years ago. In any event it is hard to imagine that the problem has ever been more severe than it is in the 1950's. Its basic cause—rising costs—may be longstanding, too. Yet it is also hard to conceive that costs have ever risen so markedly, in contrast to income, as they have in this field in recent years, in particular over the past decade. Before examining the effect of this rise on publication costs a word is in order about its effect on the costs of preparing historical materials for publication. It is well to link the costs of preparation with those of publication; otherwise too much comfort will be given to those foundations where it is now the fashion to divide these problems, on the dubious assumption that each may then be conquered separately.

As archival and editorial enterprise have improved, as shown by Mr. Butterfield, we have found ourselves faced with the higher costs of the improved performance now expected of our scholars. Mr. Berkeley has suggested gains in the problem of control which may in the long-run effect savings in research, but that day is not yet with us. The costs of reproducing materials is greater today than it has ever been, even as we enjoy the benefits of the technological developments described by Mr. Tate. In each of these steps, the achievement of new standards must not be discounted and the gains for scholarship are enormous. But it can be fairly stated that the price tags on this kind of scholarly endeavor have also reached new highs.

The price tags on the costs of publication can only cause shudders. Consider the costs of doing business today as a university press, beginning with production costs. By production costs is meant the out-of-pocket expenditures for the composition, presswork, paper, and binding necessary to produce one or more printings of a book. The sharp increase in these costs in the past ten years has kept pace with the national increase in the cost of all materials and labor—and the latter is by far the big item in the small editions which necessarily characterize scholarly publishing.

Comparative figures drown from the files of the Yale Historical Publications, Studies and Miscellany, offer some evidence in this matter. Such comparisons may not be regarded as strictly precise, as no two books are ever exactly alike, and it may also be noted that the two examples used

[1] Stevens, David H., Scholarly publishing and the foundations, *Publishers' Weekly* **151**: 524, 1947.

are not made up of purely historical source material. But care was taken to select volumes with production problems akin to those of source material, as well as comparable to each other in length, apparatus, and size of printings.

In 1941 the Yale University Press published Stetson Conn's *Gibraltar in British Diplomacy in the Eighteenth Century* in an edition of one thousand copies at a unit production cost of $1.32 per copy and priced it at $3.00. In 1952 the Press brought out William H. Jordy's *Henry Adams: Scientific Historian*, also in a first printing of a thousand copies, at a unit cost of $2.81 per copy and priced it at $5.00.[2] Each book is approximately 350 pages long.

From these figures—and there is reason to believe every other major university press could match these examples—it is painfully clear that while book production costs have more than doubled, the scholarly publisher, aware of consumer resistance, has seldom had the nerve to double the retail prices of his product, even though the automobile, shirt, and meat producers have had no such qualms. A recent survey showed that retail prices of all books, including fiction and other fields not generally the concern of scholarly publishers, have risen only 35 per cent in the past twelve years.[3] This squeeze has had the obvious effect: commercial publishers, obliged to show a profit, have had to curtail their interest in the publication of editions with limited market possibilities, while the university presses, which cannot make ends meet without subsidies, have had to look around for more subsidies.[4]

The scholarly publisher's other costs have shot up too. It costs more today, much more than it did in 1942, to buy advertising space, to print and mail circulars, to pay staff salaries, to pay rent, heat, and light (and at many presses these last are not supplied free by the parent institution), and to buy office supplies. It cost over $200,000 to operate the Yale University Press last year, exclusive of the production costs of its books and its royalties to authors.

The director of the University of California Press recently sent his scholarly authors an admirable brochure designed to explain the facts of life to them. He did so, he said, because too many of them thought that his press could manufacture a book for $1.25, sell it for $2.00, and make a profit of 75¢. Here are the facts, as he set them forth:

> If a scholarly book is of enough general interest to sell mainly through bookstores, the retail price must be set by the publisher at about four times the production cost. Thus the $5.00 you pay for a book may be distributed as follows: $2.00 to the bookseller; $1.25 to the printer; 90¢ for salaries, shipping, storage, depreciation, bad debts, telephone bills, etc. —in other words, operating overhead: 50¢ to the author; 40¢ for advertising and promotion. [This totals $5.05.] The rest is profit.[5]
>
> Textbooks and reference books [he goes on to say], which can be distributed primarily by mail, are not discounted so heavily and are less expensive to advertise. Hence they may be sold at only two or three times the production cost. Most university press books, those which are particularly expensive to make or which have a small potential market, *must* be sold this way.

In this recital of present-day high costs of scholarly publication, I have not felt it necessary to remind this audience that American university presses are operated without profit as a motive, that their primary obligation is to scholarship, and that the fruits of scholarship are in the main of such a specialized nature that the techniques of mass production and distribution for which our country has become famous cannot be brought to bear. Nor need I remind you that most university presses have assumed certain other obligations and that fulfillment of these cannot be purchased cheaply.

I mean the obligation to offer the scholar, whether he be author or editor, creative editorial guidance, as well as assistance in avoiding errors of style, clarity, and content. As the director of the University of Oklahoma Press has observed, "Contrary to a growing opinion, skillfulness in form is not merely an ornamental feature of scholarly writing. The public insists on it, and in this it is delivering an aesthetic judgment. The alternative to conformity to this judgment is neglect."[6] Such neglect can overtake scholarly editors as well as scholarly writers and I remark also on another advantage to such conformity: any

[2] The ratio of this price to production cost is not typical of the pricing formula used by the Yale University Press and other major presses. A departmental subsidy was used to hold the price within the reach of its audience; the same was true in the case of the Jordy volume. Had this not been done, the Conn book would have been priced at $5.00 and the Jordy study at $9.00.

[3] *Publishers' Weekly* **165**: 35, 1954.

[4] Kerr, Chester, *A report on American university presses*, 234 ff., Princeton, Assoc. of Amer. Univ. Presses, 1949.

[5] Frugé, August, *Publisher to author*, 9, Berkeley and Los Angeles, Univ. of Calif. Press, 1953.

[6] Lottinville, Savoie, *Southern Methodist University Studies*, **4**: 85, Dallas, Univ. Press in Dallas, 1948.

experienced scholarly publisher will assure you that a capable author or editor repeatedly saves money, whether in the initial preparation of the manuscript or in the inevitable process of altering proof, and thus helps conserve our total publication resources.

Returning to the obligations of a university press, I mean the obligation to make good design a part of the manufacture of its books, and by good design I have in mind good printing as an aid to reader interest, as well as dressing a book in keeping with its content and purpose. And I mean the obligation to market the scholar's books, not only through successful adaptation of the distribution techniques of commercial publishers, but through the special techniques, perfected alone or in company with other university presses, by which scholarly publishers seek out the limited audience for a limited book.[7]

If the scholarly publisher continues to recognize the obligations outlined in the foregoing he is manifestly going to need more money than he has been able to command in the recent past. Before offering some answers to the question of where he is going to find this money, let me present some facts and figures on four recent major historical publishing projects to illuminate further the nature of the problem. Some of this information is undoubtedly known to you but it may promote useful knowledge to record it here.

First, the monumental *Papers of Thomas Jefferson* edited by Julian P. Boyd and published by the Princeton University Press.[8] This project was formally launched by the Thomas Jefferson Bicentennial Commission in 1943 and an editorial office established at the Princeton University Library the following year. It is expected to take final form in fifty-two volumes. The first volume was published in May, 1950, and the eighth will be brought out next month. The costs of gathering and preparing the material for publication have been borne to date by Princeton University out of a gift of $200,000 made to the University for this purpose by the New York Times Company. The question has already been raised as to whether, despite stringent economy on the part of the editor and his staff, this sum will be sufficient to finance the preparation of all the remaining volumes.

The costs of publication, and they are substantial, were assumed by the Princeton Press in 1949. The Press enjoys a separate corporate identity from its parent institution and is using its own working capital for this purpose, of which a considerable amount was expended as initial advertising and promotion outlay to launch the early volumes.[9] Each volume is sold singly and has been priced to date at $10.00. A discount of 10 per cent is offered to subscribers who need only buy each volume as it appears and who may cancel their subscriptions at any time. There are over one thousand subscribers at the present time. Three thousand copies have been printed of each of the first seven volumes and 2,500 of Volume VIII. Volume I has been reprinted.

Moving north, we pause at the Rutgers University Press to examine its recent publication of *The Collected Works of Abraham Lincoln* under the editorship of Roy Bassler.[10] This project, a dream for many years of the Abraham Lincoln Association of Springfield, Illinois, may be said to have taken positive form in 1946 when Mr. Bassler's services were retained. The costs of editorial preparation were approximately $100,000, of which some $46,000 was borne by the Association while the remainder was put up by the Rockefeller Foundation.

All eight volumes of this edition, save only the index volume which is still to come, were published as a set in February of this year at a price of $115.00 and are sold only as a set. There was no attempt to attract subscribers but purchasers were offered a pre-publication price of $95.00 and some 1,100 institutions and individuals placed their orders at this price. Thirty-three hundred sets were printed. The costs of publication were underwritten by the Rutgers University Press which functions as a department of its University, meaning that in this case the parent institution bore the ultimate financial responsibility for capitalizing the venture. I am happy to report that the initial outlay for outside printing and advertising was recovered from the proceeds of sets sold before publication.

At New Haven in 1937 there were published the first two volumes in the *Yale Edition of the Correspondence of Horace Walpole* under the gen-

[7] Kerr, Chester, *ibid.*, 133 ff.

[8] The information on this project was supplied by Mr. Boyd and by Norvell Samuels, acting director of the Princeton University Press.

[9] The Princeton University Press has its own printing plant and thus some of the production costs of the Jefferson papers are a matter of bookkeeping rather than out-of-pocket expenditures.

[10] Information on this venture was supplied by Harold Munger, former director of the Rutgers University Press.

eral editorship of Wilmarth S. Lewis.[11] Work on the project was first begun in 1933. It is expected that the edition will run to fifty volumes, of which sixteen have appeared to date in batches of two or more. Seven hundred and fifty of each of the first eight volumes were printed and it is pleasant to note that beginning with the ninth volume the printing had to be increased to 900; the Oxford University Press in London has from the beginning taken from 100 to 250 of each volume for distribution in its territory. It is not so pleasant to report that on the occasion of the publication of the eleventh and twelfth volumes in 1948 it became necessary to increase the original price of $7.50 to $10.00, including the previous ten volumes. Volumes may be bought as issued. A subscription list has been built up to number approximately 350; subscribers may cancel out at any time.

The extensive costs of preparing and publishing this edition have been subsidized to date by Yale University out of private gifts made to it for this specific purpose. Thus the Yale University Press, a separate corporate entity, does not incur any financial risk but in return must pay back 50 per cent of the income from sales to the University for deposit in the original fund. This fund may thus be said to revolve, although given the proportion of costs to sales it revolves always in a descending spiral.

Finally, moving still farther north to Cambridge, we may take account of *The Letters of Theodore Roosevelt*, being published currently by the Harvard University Press as edited by Elting Morison.[12] This project was initially launched in 1946, although the full editorial staff did not begin work until 1948. It is sponsored by the Roosevelt Memorial Association which has spent to date $110,000 on editorial costs, not including housing and other free housekeeping services provided by the Massachusetts Institute of Technology. When completed the edition will consist of eight volumes issued in pairs of which the first pair was issued in 1951 with the last to come in the spring of 1954. The volumes may be purchased only in pairs at $20.00 the set. An initial attempt to attract subscribers at $70.00 for all eight volumes was only partially successful. The first four volumes were printed in editions of 2,500 each; the last four are limited to 2,000. Capital to underwrite publication costs has been provided by Harvard University of which the Press is a department.

Surely the figures offered on the costs of these four great modern works of scholarship would seem fabulous to the thrift-minded Founder of this Society. Yet these costs are bound to be dwarfed by the expenditures which must be met when comparable major projects are undertaken in the future, of which at least two are in immediate prospect. Those charged with the responsibility for these new projects can learn many useful and sobering lessons from the experiences of those who had to plan and finance the costs of the editions described in the foregoing.

Two other observations. The first is that during our century the main responsibility for publication of such material has apparently passed from the hands of the commercial publishers to the university presses. It was the Macmillan Company, for example, which in 1905–1907 isued the latest complete edition of the writings of Benjamin Franklin.[12a] The Smyth edition ran to ten volumes and was first published in a so-called limited edition of a few hundred copies at a price of $30.00 per set. A trade edition, also in ten volumes, was issued from the same plates in 1907 and was priced at $15.00 the set. Fifteen hundred copies of the latter edition were printed and it took fifteen years to sell it out.[13]

The second observation is that the scholarly publishers are meeting this responsibility grandly—if I may use a word that signifies my prejudice—and that in three of the four cases noted the costs of publication have been underwritten by the universities concerned or by their publishing arms, rather than by those who sponsored the massive costs of the preparation of the manuscripts.

These are heartening developments, if you will, and they point to a conclusion evident for some time to many scholarly publishers: that it is to the universities themselves that American scholarship must look for those essential publication funds of which Mr. Stevens spoke. If this is so, it may be well to review the sources of funds available to our major universities, private or state, and what may be more pertinent, to their publishing houses. I see three possible sources of such funds: (1) sav-

[11] Data about this project came from the files of the Yale University Press.

[12] Information furnished by Thomas J. Wilson, director of the Harvard University Press.

[12a] Smyth, Albert Henry, *The Writings of Benjamin Franklin*, New York, Macmillan, 1905.

[13] Facts supplied by George P. Brett, president of the Macmillan Company.

ings; (2) expanded markets; and (3) new subsidies.

I have three kinds of savings in mind. First, the kind that results when a university makes certain that its press is being run as efficiently and therefore as economically as possible—in other words, the savings in operating costs which can result from good management. Three years ago at the Yale University Press we sharpened the pruning shears and cut $15,000 off our annual operating expenses, without, so far as we can discover, in the least impairing the services we perform. Fifteen thousand dollars, I need scarcely add, will subsidize the publication cost of several volumes of historical source material.

A second kind of saving is the one that may result, although it has not yet proved to be entirely true, from the adoption of new or substitute manufacturing methods. The alternative methods of composition, for instance, such as IBM typewriter or Varityper, have yet to prove themselves as means of effecting large economies, and the recent inventions in the field of photo composition have not yet demonstrated their worth conclusively. But university presses must watch these developments carefully and keep an open mind. Neither they nor their scholarly authors can any longer afford the luxury of the attitude that a book must be printed by letter-press or not at all.[14]

A third kind of saving is suggested by raising the fundamental question of whether certain scholarly material of an extremely limited nature should be printed at all in book form. With costs where they are now, the heresy may again be advanced that it is wasteful of our society's resources to print any book for which there is a market of less than 1,500 copies. Microfilm or microcard or some other substitute form of reproduction may be sufficient.

I spoke also of expanded markets for their publications as a source of additional income for university presses. During the past twenty years sales income has assumed its rightful place as their single largest source of income.[15] Scholarly publishers have learned how to sell their books with a competence which matches the commercial houses and have perfected some special selling skills of their own. But there is still room for improvement and additional sales income is clearly one way, and perhaps the most satisfactory way, that presses can help solve the problems of financing scholarly publication.

Another possible source of revenue under the heading of expanded markets is recourse to the paper-bound reprint edition. This revolution in American book publishing—and it cannot be described otherwise—has just begun to affect scholarly publishers and its implications are far from clear, except that the temptation to broaden the audience for a book is tremendous and it is already evident that more and more scholarly publishers are going to be approached by paper-bound reprint houses in search of serious items for their lists.

Thirdly, to expand his markets, the scholarly publisher can engage in a greater amount of trade publishing—that is, publishing of titles of a more general interest—as many of them have already done. "The profit road," Donald Bean, then director of the University of Chicago Press, called it in 1933—a path sometimes followed by a press to "enlarge its scale of commercial ventures in the hope that larger profits might finance the publication of the increasing number of admittedly unprofitable scholarly projects."[16] And then Mr. Bean, who is a wise man and now one of the elder statesmen of our profession, went on to point out the dangers of this path. "There are," he said, "real obstacles to overdevelopment of this type of publishing under university auspices: (1) larger outlay and investment, more risk, and not always greater profit; (2) competition with commercial agencies; (3) and undue proportion of commercial publishing might overshadow the distinct object of a university press."[17]

Finally, we come to the third and not least of the additional sources of revenue which may make it possible for universities and their presses to alleviate this problem of financing the publication of historical sources as well as other material rightfully in its domain. I mean subsidies. In this connection, far less attention must be paid to the scholar himself as a source of subsidy. Surely it is grossly unfair that so often today the scholar, particularly the younger man who can presumably least afford it but has no choice if he is to win promotion, is obliged to help pay the cost of publishing his book, either directly by underwriting

[14] Major assistance in their thinking on these matters was made available through the admirable but short-lived advisory service on publications maintained by the American Council of Learned Societies in 1949–1951.

[15] Kerr, Chester, *ibid.*, 236–239.

[16] Bean, Donald P., *The University of Chicago Survey* 3: 259, Chicago, Univ. of Chicago Press, 1933.

[17] Bean, Donald P., *ibid.*

part or all of its publication cost, or indirectly by foregoing royalties on at least the first printing.

Some attention should undoubtedly be paid to endowments, a possibility which has been exploited by several presses, notably my own where a system of endowment built up skillfully over the years by its founder enabled the Press until last year to remain free of any general subsidy from its parent institution. The extent to which it continues to remain free will depend on the success of the fund-raising officers at Yale who have included a million dollars for the Press in the University's recent appeal for additional general endowment of $80,000,000.[18]

Just what can be expected from the foundations in support of scholarly publication during the remainder of our century remains a moot point. For reasons best known to themselves, the major institutions among them apparently subscribe to the belief that it is proper to finance research, even for years at a time, without making available funds to publish the results. In Walter Muir Whitehill's paraphrase of Pinckney, their present policy seems to be, "Millions for research but not one cent of publication." [19] There are occasional happy exceptions to this attitude. It is to be hoped these will multiply.

What can be expected from government a hundred years after Jared Sparks is also not yet clear. At the rate at which the present administration in Washington is embracing economy, not to mention its anxiety to be rid of the consequences of any program undertaken by its immediate predecessors, the prospects for any substantial federal subventions through the ambitious program of the National Historical Publications Commission appear to be dim, even if they should be thought desirable.[20] Nothing would please me more, I must add, than to learn that I have misjudged the present tenor of affairs in Washington.

I come at last to the universities, specifically to the parent institutions of the two dozen leading American university presses. It is on their doorstep, I believe, that the crying need for additional subsidies for publication should properly be laid. The case for such parenthood was succinctly stated long ago by Wilbur L. Cross, at that time Dean of the Graduate School at Yale. Dean Cross said this to the annual conference of the Association of American Universities in 1921: "Scholarly publication of every kind, I asume, has the moral support of the great American universities, but it must also have their full financial support if they [the universities] are to take to the position rightly expected of them. . . ." [21]

Governor Cross would be pleased to know, I'm sure, that for the past two years his own University, now presided over by a young historian, has seen fit to appropriate for the first time since its Press was founded in 1908 the sum of $10,000 annually as additional funds for the publication of research performed by its own faculty members, thus releasing other Press publication funds for outside projects. He would also be gratified to learn that when presented with the opportunity to share in a momentous new project in the field of American historical documents his University was able to command the considerable resources necessary to join in the enterprise and that this occurred at the very moment it was having little luck with its appeal to similar sources for funds to erect a new physics laboratory.

This last may also comfort Abraham Flexner who recently struck so vigorous a blow on behalf of the neglected humanities. "It is imperative," wrote Dr. Flexner, "that universities cease to waste money upon luxuries, superfluities, extravagant buildings, and nonsensical subjects and departments in order to concentrate upon their indispensable concerns." [22] I submit that among those indispensable concerns belongs wholehearted financial support of university publication of the main historical sources of our national heritage, in particular, in Mr. Truman's words, of the "need to collect and publish the writings of men and women who have made major contributions to the development of our democracy." [23]

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[18] *Yale plans for the future*, New Haven, Office of Univ. Development, 1950.

[19] Whitehill, Walter Muir, The break in the circle, *Huntington Library Quart.* 3: 235, 1953.

[20] *A national program for the publication of the papers of American leaders*, Washington National Historical Publications Commission, 1951.

[21] Cross, Wilbur L., Concerted action toward full support of scholarly publication, *Jour. of Proc. and Addresses of the Assoc. of Amer. Universities*, 33, 1921.

[22] Flexner, Abraham, *Funds and foundations*, 140, New York, Harper and Brothers, 1952.

[23] *A national program for the publication of the papers of American leaders*, Washington National Historical Publications Commission, 1951.

THE REDISCOVERY OF PEALE'S MASTODON

GEORGE GAYLORD SIMPSON and H. TOBIEN

American Museum of Natural History

Geologisch-Mineralogische Abteilung des Hessischen Landesmuseums

(Read by Dr. Simpson, April 23, 1954)

One of the most interesting and historically most important of all fossils was the mastodon skeleton found in 1799 in Orange County, New York. It was collected by Charles Willson Peale and others in 1801. It was restored and mounted and was exhibited in Peale's Philadelphia Museum, first in the Hall of the American Philosophical Society and later in the building now known as Independence Hall. It was the first essentially complete mastodon discovered, the first fossil skeleton of any kind to be mounted in America, and among the first (possibly the second) in the world. (See Simpson, 1942, and references there.) The elder Peale died in 1827. Other members of the family tried to maintain his museum for a time, but it had numerous financial and other vicissitudes. The natural history collections were for the most part purchased by P. T. Barnum in 1850. Barnum's Philadelphia Museum burned in 1851 and his American Museum in New York (not connected with the present American Museum!) burned in 1865.

Historians have always assumed that the famous first or Philadelphia mastodon skeleton perished in either the Philadelphia or the New York fire in Barnum's museums. That was definitely stated by Harold Sellers Colton, a leading student of the Peale family (e.g., Colton, 1909) and has been accepted on his authority by all subsequent students, as far as we know. Fortunately, however, it is wrong. The first Peale skeleton still exists. It is in the Geologisch-Mineralogische Abteilung des Hessischen Landesmuseums in Darmstadt, Germany, for which it was purchased by J. J. Kaup in 1854. Although this fact has been so generally overlooked, it was mentioned by Kaup in publication (1857) [1] and in letters. A full account is given in an undated and unsigned report in the Museum archives (Schleiermacher Registratur Abt. IV Konv. 682 fasc. Museum.) written at some time in the 1860's. A translation of some of the pertinent parts of the report follows:

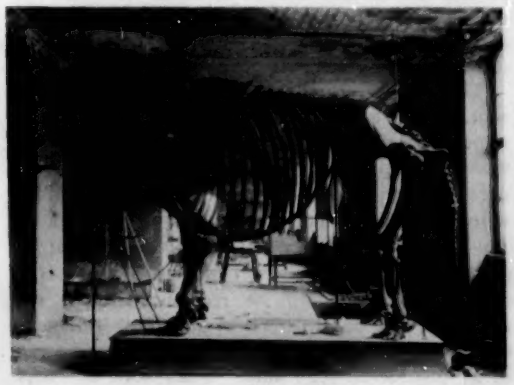

Fig. 1. The Peales' first or Philadelphia skeleton of the American mastodon as preserved in Darmstadt in 1953.

> In the Grand-ducal Museum there is set up the skeleton of a prehistoric animal, known under the name of Ohio-animal or flesheating elephant and named *Mastodon* by the naturalists. This skeleton comes from North America, was originally set up in the Peale Museum in Philadelphia, and was described and figured by the owner (Warren) [see comment below]. . . .
>
> This skeleton, found in 1801, was shipped to Europe by speculators and was sold for 100,000 frs. to King Louis Philippe, who planned to give it to the Jardin des Plantes. They came to Boulogne a few days too late with the skeleton, because the king had already started on his flight to London. They therefore returned with the skeleton to London, where they offered it to the British Museum for 24,000 fl. But here, too, they came too late, for the British Museum had bought a skeleton from Koch for 12,000 fl. a short time before. . . . Some time later they offered the skeleton to the Keeper of Skeletons at the College of Surgeons for 9,000 fl., whereupon Prof. Owen offered 6,000, which they did not accept. . . .

[1] Kaup's brief published mention states that he purchased the skeleton in 1855. Other data (available by courtesy of Drs. Hefele and Heldmann, Zoological Dept. of the Hessisches Landesmuseum, who are preparing a biography of Kaup) show that Kaup did not visit London in 1855 and that the date of purchase was 1854.

> After that time it was exhibited in London at three different places, last in a Glass Hall of the Colosseum, where it fell into complete neglect because the similar specimen of Koch could be seen for nothing at the British Museum. Our Inspector Dr. Kaup rescued it from this neglect by purchasing it for 1,200 fl. on his last visit to London. . . . [There follow rather lengthy remarks on what a bargain Kaup's purchase was.]
>
> . . . As with all remains of prehistoric animals, everything cannot be original and . . . the fragile top of the skull, the lower jaw, the 10th [*sic!*] neck vertebra, most of the ribs, and the right thigh bone are defective or wholly restored. . . .

This account is consistent with the first Peale mastodon except for the statement that it was described and figured by the owner ("Besitzer") Warren. Warren acquired the second and less complete of the two skeletons collected and restored by the Peales, the Baltimore skeleton exhibited in Rembrandt Peale's museum in that city. That skeleton is now in the American Museum of Natural History (no longer mounted) and cannot be the one in Darmstadt.

Warren (1852 and later editions of the same work, upper left figure of the frontispiece) did publish a lithograph of the first Peale mastodon. He said of it,

> In Peale's Museum at Philadelphia, it occupied a prominent place, until, in the year 1849 or '50, it disappeared; and since then, there is no authentic account of its history. (Warren, 1852: 212.)

In the following paragraph Warren wrote that,

> In the summer of 1851, being in Paris, I was told by M. Laurillard, of the Jardin des Plantes, that a Mastodon skeleton had been offered for sale in that city; but Professor de Blainville found it in so disordered a state, that he would not advise its purchase. Soon after, it disappeared, and he knew not in what place it then reposed.

In the second edition of his monograph Warren (1855: 252–253) repeated the statement that Peale's [first] mastodon disappeared in 1849 or 1850 and had no authentic subsequent history. Of the skeleton offered for sale in Paris, he now added that, "This skeleton, I have reason to believe, is now in the possession, or at least under the direction, of my friend, Prof. Kaup, in the Museum of Darmstadt, in Germany." Warren may have suspected that the skeleton that finally went to Darmstadt was actually Peale's first skeleton. At least, the juxtaposition of the statements could be so read: Peale's skeleton disappeared without later history and one appeared without earlier history in Paris one or two years later.

We believe that these skeletons were, indeed, the same although there are some discrepancies. Warren says that Peale's first mastodon was in Philadelphia until 1849 or 1850, but the Darmstadt record says that the specimen now there was taken to France at the time of Louis Philippe's flight from that country, which occurred in 1848. Warren does, however, confirm other statements that the specimen later in Darmstadt was offered for sale in France prior to the summer of 1851. Warren's "1849 or '50" for the disappearance of Peale's mastodon from Philadelphia implies uncertainty as to the date, and probably he was in error. The skeleton may well have disappeared from Philadelphia in 1848 or possibly even earlier and so could have been the one offered to Louis Philippe in 1848, as we believe it was.

There is no serious reason to question the explicit statements by Kaup and in the Darmstadt Museum's records that the Darmstadt skeleton had belonged to the Peales. It could not have been the Peales' second, or Baltimore, mastodon, the history and present whereabouts of which are fully known. Therefore it was the first, or Philadelphia, mastodon. This is largely confirmed by the specimen, itself. It seems to include the parts specified by Rembrandt Peale (1803) as present in the first mastodon and to lack those then noted as absent. Most noteworthy is the absence of the lower jaws, present in the Peales' second mastodon but said by R. Peale to have been carved from wood to restore the first skeleton. The specimen now in Darmstadt does have lower jaws carved from wood. Genuine molar teeth of one or more other individuals have at some time been rather unskillfully inserted in the wooden jaws.

The skeleton as now preserved does not represent Peale's original mount and it was evidently remounted, probably under Kaup's direction, after it arrived in Darmstadt. The present mount does not agree in posture or in details of restored parts with Warren's lithograph of the Peale mount. The tusks were also newly restored with papier maché in which political broadsides of the 1850's in Darmstadt can still be recognized. The remounting may be explained by Warren's statement that what we believe to be the same specimen was in a "disordered . . . state" when seen in Paris prior to Kaup's purchase.

The historic specimen survived World War II with no damage except that caused to the papier

maché tusks by rain. Since the present mount is not that made by the Peales (it is in some respects less accurate), it is planned to replace it with a modern mount. The new mount will still incorporate the original bones of the Philadelphia skeleton, now rediscovered and entering its fourth incarnation.

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LINGUISTIC PREHISTORY OF INDIA

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(*Read April 23, 1954*)

At some time in the second millennium B.C., probably comparatively early in the millennium, a band or bands of speakers of an Indo-European language, later to be called Sanskrit, entered India over the northwest passes. This is our linguistic doctrine which has been held now for more than a century and a half. There seems to be no reason to distrust the arguments for it, in spite of the traditional Hindu ignorance of any such invasion, their doctrine that Sanskrit is "the language of the gods," and the somewhat chauvinistic clinging to the old tradition even today by some Indian scholars. Sanskrit, "the language of the gods," I shall therefore assume to have been a language brought from the Near East or the Western world by the nomadic bands.

These invaders did not penetrate into a linguistic vacuum. What did they find to the east of the passes? If the archaeologists' reconstruction and chronology of the events are at all well-founded, they found over the whole of the Indus Valley a high culture (the Harappā culture of the archaeologists), which was a sibling or a remote cousin of the high cultures of the Near East—Egypt, the Fertile Cresent, Mesopotamia.[1] Here were great and small cities, whose life was based on the riverine agriculture permitted by a great river and its tributaries in a vast, irrigable, alluvial valley. The probabilities are that the culture was as rigidly regimented and as firmly based on a system of serfdom or slavery as were the Near Eastern cultures.[2] So far the excavations have not yielded, and it seems improbable that they will in future yield, evidence of a material brilliance such as that of Egypt and Mesopotamia. It may be that the Harappā culture was the "repellent" thing that one of its interpreters[3] has called it—"drab" or "dull"[4] might be a better, because a less subjective, word.

For our present interest, however, it is to be recalled that this culture was literate. The many short inscriptions on seals are written in a script that is non-identical in its forms with either the Egyptian hieroglyphs or the Mesopotamian cuneiform, but that is somewhat similar to the early stages of both in that it probably developed from a picture-writing, rebus-like in nature, and was apparently, like both, syllabic in its structure.[5] The unknown script and subject-matter, combined with the shortness of the inscriptions, have so far defied convincing decipherment.

The most ambitious attempt up to date, that of Father Heras,[6] has posited that the language is an old member of the Dravidian family which is now located chiefly in South and Central India, but which still has an outlying member, Brahui, in the highlands of Baluchistan to the west of the Indus Valley. There is nothing *a priori* against this assumption. Speakers of Dravidian languages now number about 90 millions or just under one quarter of the population of the Indian subcontinent (making the family the fifth or sixth largest in the world). The geographical distribution, and the nature of the boundary in Central India between Dravidian speakers and the speakers of the Indo-Aryan languages that descend from the invader language Sanskrit, are good evidence that Dravidian has been steadily retreating before Indo-Aryan. The Dravidian-speakers of the farthest South, the Tamil-

[1] See, e.g., Mackay, Ernest, *Early Indian civilizations*, London, Luzac and Co., 1948; Wheeler, R. E. M. (now Sir Mortimer), in various places, esp. *Ancient India* **3**: 74–76, Jan. 1947, and *The Cambridge history of India*, supp. vol. *The Indus civilization*, Cambridge, Univ. Press, 1953; Piggott, Stuart, *Prehistoric India to 1000 B.C.*, Pelican Book, Harmondsworth, Middlesex, 1950, with good select bibliographies.

[2] Wheeler, *Ancient India*, *loc. cit.*

[3] Piggott, *op. cit.* 201.

[4] "Dull" is Wheeler's word in *Tamil culture* **2**: 20, 1953; he declares, however, in *The Indus civilization*, 63 f. that the remains suggest "an aesthetic capacity more broadly based than the recovered examples of it alone would indicate," an aesthetic capacity perhaps expended in part on the perishable art of wood-carving.

[5] Piggott, *op. cit.* 178–181. Kroeber's term "stimulus diffusion" must be called to mind.

[6] Many articles in Indian journals have been devoted to this "decipherment." It seems unnecessary to list them.

speakers, early acquired a high culture with all the features of the North Indian culture that was framed in Sanskrit. Even the grammer and the literary criticism of the Sanskritic culture were among the early borrowings and are the subjects of the earliest extant Tamil text. As is usual in India, no exact dating of this early literature is possible, but it is clearly no later than the earliest centuries of the Christian era and may well be pre-Christian. Tamil script is known from the Arikamedu finds of the first century A.D. And the presence of Aśokan inscriptions in Middle Indo-Aryan dialects as far south as Mysore State in the third century B.C. is witness to the spread of Sanskritic culture southwards prior to this period—for who could have been addressed by Aśoka in Indo-Aryan dialects if not literate brahmans who had settled in the South in some numbers on their missionary occasions?

Be that as it may, it has been claimed that the far southern, Tamil form of the brahmanical Sanskritic culture of India shows in its earliest remains so many specific high features that are not North Indian, that we must posit a high culture in South India prior to the spread of the brahmanical culture there. Archaeological evidence to substantiate these claims, however, is still lacking.[7] Yet, taking all together, the assumption that the language of the Indus Valley documents was Dravidian is clearly not fantastic.

In spite of this, the attempts at interpretation have not been convincing. Since the subject-matter of the documents is unknown, any subject-matter, i.e. meaning, that is assigned to any element of the script, is arbitrary. A succession of arbitrary meanings thus assigned to the symbols (with, to be sure, a little aid from Sumerian texts) may, in fact, be made to coincide with a Dravidian-like succession of phonemes, but only for short stretches of the material. The Indus Valley material is made up of a fairly large number of short inscriptions, but it is only by positing numerous arbitrary variations of sound and of meaning for the same symbols that anything like a coherent system can be made to run through the total corpus. It is this necessity to vary, or, in other words, this self-given permission to vary in the sound and meaning that are attached to the smallish number of different symbols, plus the impossibility of checking the arbitrary meanings, that has convinced the interested scholars that the method used has been an unconvincing one—that, to put it more crudely, by this method one can give the inscriptions any interpretation one wishes.

In fact, promising as it has seemed to assume Dravidian membership for the Harappā language, it is not the only possibility. Professor W. Norman Brown has pointed out (*The United States and India and Pakistan*, 131–132, Cambridge, Harvard University Press, 1953) that Northwest India, i.e. the Indus Valley and adjoining parts of India, has during most of its history had Near Eastern elements in its political and cultural make-up at least as prominently as it had true Indian elements of the Gangetic and Southern types. The passage is so important that it is quoted in full:

> More ominous yet was another consideration. Partition now would reproduce an ancient, recurring, and sinister incompatibility between the Northwest and the rest of the subcontinent, which, but for a few brief periods of uneasy cohabitation, had kept them politically apart or hostile and had rendered the subcontinent defensively weak. When an intrusive people came through the passes and established itself there, it was at first spiritually closer to the relatives it had left behind than to any group already in India. Not until it had been separated from those relatives for a fairly long period and had succeeded in pushing eastward would it loosen the external ties. In period after period this seems to have been true. In the third millennium B.C. the Harappa culture in the Indus Valley was partly similar to contemporary western Asian civilizations and partly to later historic Indian culture of the Ganges valley. In the latter part of the next millennium the earliest Aryans, living in the Punjab and composing the hymns of the Rig Veda, were apparently more like their linguistic and religious kinsmen, the Iranians, than like their eastern Indian contemporaries. In the middle of the next millennium the Persian Achaemenians for two centuries held the Northwest as satrapies. After Alexander had invaded India (327/6–325 B.C.) and Hellenism had arisen, the Northwest too was Hellenized, and once more was partly Indian and partly western. And after Islam entered India, the Northwest again was associated with Persia, Bokhara, Central Asia, rather than with India, and considered itself Islamic first and Indian second.
>
> The periods during which the Punjab has been culturally assimilated to the rest of northern India are few if any at all. Periods of political assimilation are almost as few; perhaps a part of the fourth and third centuries B.C. under the Mauryas; possibly a brief period under the Indo-Greek king Menander

[7] Wheeler, *Ancient India* 4: 89, July 1947–Jan. 1948, with some bibliography.

in the second century B.C.; another brief period under the Kushanas in the first and second century A.D.; an even briefer period under the Muslim kingdom of Delhi in the last quarter of the twelfth century A.D.; a long one under the great Mughals in the sixteenth and seventeenth centuries A.D.; a century under the British, 1849–1947.

Though this refers to cultural and political factors, it is a warning that we must not leap to linguistic conclusions too hastily. The early, but probably centuries-long condition in which Sanskrit, a close ally of languages of Iran, was restricted to the northwest (though it was not the only language there) and the rest of India was not Sanskritic in speech, may well have been mirrored earlier by a period when some other language invader from the Near East—a relative of Sumerian or of Elamitic or what not—was spoken and written in the Indus Valley. It is not ruled out, of course, that this anonymous Near Easterner might have been only one of the languages spoken in the Indus Valley—perhaps that of invaders and conquerors—while the indigenous population spoke another language—perhaps one of the Dravidian stock, or perhaps one of the Munda stock, which is now represented only by a handful of languages in the backwoods of Central India.

On leaving this highly speculative question, we can move on to an examination of the Sanskrit records, and we find in them linguistic evidence of contacts between the Sanskrit-speaking invaders and the other linguistic groups within India.

Whenever two language communities come in contact and remain in contact for any appreciably long period, the languages have some effect upon each other's structure. Borrowing of words in one or the other direction or in both is the most obvious effect. But there may also be a shift of sound systems, borrowing of derivational or inflectional morphemes, or borrowing of syntactical traits.

Sanskrit scholarship in the West soon saw that some of the non-Indo-European features of Sanskrit were Dravidian (or possibly Munda) in type. The retroflex (domal or cerebral) consonants in Sanskrit may be explained for some of their occurrences as being the reflexes of Indo-European consonant clusters of certain types. The fact, however, that the later in Indo-Aryan linguistic history we go, the greater is the incidence of retroflex consonants, and the further fact that most of the Dravidian languages and Proto-Dravidian itself have this type of consonant in abundance (the case is not so clear for Munda, but is in all probability similar), can only lead to the conclusion that the later Indo-Aryan developments are due to a borrowing of indigenous speech habits through bilingualism, and to the well-grounded suspicion that even the early development of retroflexes from certain Indo-European consonant clusters results from the same historic cause.[8] The same argument applies also to the development of absolutives (otherwise called gerunds) in Sanskrit; this non-finite verb form and its syntactic use are so closely parallel to a feature of Dravidian and so unlike what is found in the other old Indo-European languages that we must certainly posit Dravidian influence here.[9]

Prima facie, it should have been easy to examine the Sanskrit dictionary for possible borrowings from Dravidian. There were, however, several blocking factors. First and perhaps most important was the assumption, usually but not always only implicitly made and seldom argued or supported by evidence, that the Sanskrit-speaking invaders of Northwest India were people of a high, or better, a virile, culture, who found in India only culturally feeble barbarians, and that consequently the borrowings that patently took place from Sanskrit and later Indo-Aryan languages into Dravidian were necessarily the only borrowings that could have occurred. Indian civilization itself, with its enthronement of Sanskrit at the expense of other languages, taught Western scholars to think this way about Sanskrit. Moreover, the early days of Indo-European scholarship were without benefit of the spectacular archaeological discoveries that were later to be made in the Mediterranean area, Mesopotamia, and the Indus Valley. It was but natural to operate with the hidden, but anachronistic, assumption that the earliest speakers of Indo-European languages were like

[8] This doctrine is held by, e.g., Bloch, Jules, Sanskrit et dravidien, *Bull. Soc. Ling. Paris.* **25**: 1–21, esp. 4–6, 1925; Some problems of Indo-Aryan philology, *Bull. School Orient. Stud.* **5**: 731–733, 1930; *L'Indo-aryen du Veda aux temps modernes*, 53 ff., 325, Paris, 1934; Katre, S. M., *Some problems of historical linguistics in Indo-Aryan*, 135 ff., Bombay, 1944; Prokosch, E., *A comparative Germanic grammar*, 39, Philadelphia, 1939; Wackernagel, Jakob, *Altindische Grammatik* **1**: 165, §144(a) Anm., Göttingen, 1896, with earlier bibliography, Gundert in 1869, *Ztschr. deutsch. morgenländischen Ges.* **23**: 517 ff., having apparently the earliest suggestion.

[9] *Cf.* Bloch, *Bull. School Orient. Stud.* **5**: 733–735, 1930.

the classical Greeks or Romans—prosperous, urbanized bearers of a high civilization destined in its later phases to conquer all Europe and then a great part of the earth—rather than to recognize them for what they doubtless were—nomadic, barbarous looters and cattle-reivers whose fate it was through the centuries to disrupt older civilizations but to be civilized by them. This was in all probability the event in India as it was in Greece or in the late Roman Empire.[10]

This assumption led in the long run to another block—the methodological tendency of the end of the nineteenth and the beginning of the twentieth century to attempt to find Indo-European etymologies for the greatest possible portion of the vocabularies of the Indo-European languages (see also note 10), even though the object could only be achieved by flights of phonological and semantic fancy. Latin perhaps was the greatest sufferer from this urge,[11] but none of the languages was exempt, and Sanskrit was no exception. It was the less pardonable in dealing with a language spoken and written in India, where even casual inspection of the Dravidian languages would have suggested some borrowings, at least, from Dravidian into Sanskrit.[12]

The third blocking factor has been for long the general ignorance of and indifference to the Dravidian languages, even among professed Indological linguistic scholars. They must not, of course, be judged too harshly. The Dravidian languages are not easy, most of them are languages spoken by backwoods "primitives" and are badly reported, the four literary languages have enormous literatures; *ars (et scientia) longa, vita brevis.*

Finally, a fourth blocking factor has been the general caution of Indo-European scholars when confronted with a substratum situation. In this they were justified, for apart from Basque, the substrata that have been operated with in the study of the history of European languages have been languages that are fragmentarily recorded and badly known (e.g. Gaulish Celtic), not really on record (e.g. Illyrian), or not understood (e.g. Etruscan). In the case of Sanskrit, however, the Dravidian substratum is easily accessible in its dozen or more living languages and in that a Proto-Dravidian can be worked out, given enough scholars interested in the matter.

The end result of the block, however, was that very few scholars attempted to identify borrowings from Dravidian into Sanskrit; those who were interested worked unmethodically and without establishing criteria for recognition of probable, possible, and unlikely examples, and their results were universally ignored. The Sanskrit etymological dictionary of Uhlenbeck (1898–1899)[13] and the Indo-European etymological dictionary of Walde and Pokorny (1930–1932)[14] completely ignore the work of Gundert (1869),[15] Kittel (1872, 1894), [16] and Caldwell (1856, 1875).[17]

[10] Mingled with this essentially Europe-centered attitude was that other strand in late eighteenth century thinking, the "romantic" one stressed by Mayrhofer, Manfred, *Saeculum* **2**: 54 ff., 1951. It led to Sanskrit being regarded (to use Mayrhofer's phrase) as "die ur-ste aller Ursprachen," as an exemplar of purity and freedom from all non-Indo-European influence. The Indo-European savages, in short, were the noblest of all noble savages. But this, of course, is ethnocentrism all over again.

[11] *Cf.* Lane, George S., *Lang.* **25**: 335–337, 1949.

[12] The borrowings from the vernacular Middle Indo-Aryan into Sanskrit, when the latter became, as it early did, a hieratic and then a dead language in which speakers of Middle Indo-Aryan languages composed freely, form another portion of the Sanskrit lexicon which must be identified and not subjected to fantastic Indo-European etymologizing. Paul Tedesco has made many notable contributions here, but he also is willing to operate with a methodology in which the Dravidian languages do not exist except as borrowers from Indo-Aryan. One example of this is his treatment of words for "belly, stomach" in Sanskrit piṭaka- "basket," *Archaeologica orientalia in memoriam Ernst Herzfeld*, 218, Locust Valley, N. Y., 1952. Whatever may be the etymology of Br. piḍ "belly, stomach" (so with Bray, rather than p[h]īḍ with *Linguistic Survey of India*), the Go. pīr id. is rather to be interpreted as going with Te. pēgu, prēgu "entrail, gut, bowel" than as borrowed from an Indo-Aryan *peṭṭa-. More striking is the Sanskrit word mālā- "garland, wreath," which can be provided with an Indo-Aryan etymology only with the utmost ingenuity (*Jour. Amer. Orient. Soc.* **67**: 85 ff., 1947). The "rope" meanings in modern Indic vernaculars may belong to homonyms, to be etymologized as Tedesco does. But the Sanskrit "garland" word may well be borrowed from the Dravidian group of words given in Appendix 1, etym. 13, which see for details.

[13] Uhlenbeck, C. C., *Kurzgefasstes etymologisches Wörterbuch der altindischen Sprache*, Amsterdam, 1898–1899. He mentions non-Sanskritic languages, though without any detail, s.vv. drāviḍī, pallī. Mayrhofer has apparently found several more such instances; *Saeculum* **2**: 55, 1951: "verzeichnet kaum fünf Wörter einheimischer Abkunft."

[14] Walde, Alois, and Julius Pokorny, *Vergleichendes Wörterbuch der indogermanischen Sprachen*, 3 v., Berlin, 1930–1932.

[15] Gundert, H., *Ztschr. deutsch. morgenländischen Ges.* **23**: 517–530, 1869.

[16] Kittel, F., *Indian Antiquary* 1: 235–239, 1872; *A Kannaḍa-English dictionary*, xiv–xlv, Mangalore, 1894.

[17] Caldwell, Robert, *Comparative grammar of the Dravidian languages*, 565–579, 2d ed., 1875. I have no access to the 1st ed. of 1856.

More recent work by Jules Bloch (1925, 1930, 1934)[18] attempted to salvage some items from the early attempts, and in the 40's T. Burrow in an important series of articles (1945, 1946, 1948)[19] attempted to set up methodological principles (see Appendix 1) and suggested Dravidian sources for some five hundred Sanskrit words. Collaborative work by Burrow and myself on a Dravidian etymological dictionary will add more items. The Sanskrit etymological dictionary that Manfred Mayrhofer has just begun to publish in Germany (1953)[20] takes account of this recent work.

It is clear that not all of Burrow's suggested borrowings will stand the test even of his own principles.[21] Much labor will have to be expended by qualified scholars on methods and on the individual items, but it can be safely predicted that the work of these modern scholars will yield an acceptable residue that will at long last be available for studies of Indic linguistic prehistory.

It is possible already to take some tentative steps.

As was to be expected, many of the borrowed items are names of flora and fauna indigenous in India and not elsewhere in the old Indo-European territory, e.g. the fragrant screw-pine, *Pandanus odoratissimus* (App. 1, etym. 1), the cardamom (etym. 2), the house lizard, *Lacerta gecko* (etym. 3), perhaps the peacock (etym. 4), certainly the white ant or termite (etym. 5). Some of these words occur in Sanskrit literature from the epic on; others are hardly more than items in the lexica (perhaps intended as etyma from which the Dravidian words were to be derived). But words of much more general semantic range also occur, some (e.g. nīra- "water," etym. 6) as early as the epic. We can be sure of Dravidian origin for a few proper names; e.g. the late Vedic and epic hero Nala seems to have a Dravidian name "the good (or handsome) man," and the southern Malaya mountains have a name derived from the Dravidian *malay "mountain" (etymologies 7 and 8). Even the interjection aye of the dramas probably has a Dravidian origin (etym. 9). It is unexpected to find that kalā- "an art" (epic and later), which has not been provided with an Indo-European etymology, is probably derived from the Dravidian verb kal- "learn," but the suggestion cannot be rejected out of hand and in fact is not unattractive when we remember that India had a high culture before the Sanskrit speakers arrived.

The greatest interest attaches to the items that occur in the earliest Sanskrit recorded, the Rig-Veda. Burrow finds some twenty words, a very mixed lot including the "peacock" word mentioned before and one or two other labels for specifically Indian phenomena. Most of them, however, are much more general; e.g. khála- "threshing floor" (etym. 10) and phála- "fruit" (etym. 11). kāṇa- "one-eyed" is very obviously derived from the negative adjective ("who does not see") of the Dravidian verb *kāṇ- "see."[22] Most strikingly, bála- "strength," which has been one of the more certain examples given in the handbooks for the rare Indo-European phoneme *b (though the meanings of the cognates are not really identical), may be derived from the very wide-spreading and ramifying Dravidian family of val- "be strong, strong, strength" (etym. 12); one of the languages in which Proto-Dravidian *v becomes b will have to be involved here. Indo-Europeanists may be inclined not to accept this, unless they are already desirous of getting rid of all examples of the rare Indo-European *b.

If the Rig-Vedic examples, or any of them,

[18] Bloch, Jules, Sanskrit et dravidien, *Bull. Soc. Ling. Paris* 25: 1–21, 1925; Some problems of Indo-Aryan philology: II. Indo-Aryan and Dravidian, *Bull. School Orient. Stud.* 5: 730–744, 1930; *L'Indo-aryen du Veda aux temps modernes*, 322–328, Paris, 1934.

[19] Burrow, T., Some Dravidian words in Sanskrit, *Trans. Philol. Soc.* 1945: 79–120; Loanwords in Sanskrit, *ibid.* 1946: 1–30; Dravidian studies VII: Further Dravidian words in Sanskrit, *Bull. School Orient. and African Stud.* 12: 365–396, 1948.

[20] Mayrhofer, Manfred, *Kurzgefasstes etymologisches Wörterbuch des Altindischen*, 1 und 2. Lieferung, Heidelberg, Carl Winter, 1953, 1954.

[21] One should certainly rule out the Dravidian etymologies given for kúṇāru- and vrīś-, which occur in one passage each in the Rig-Veda and nowhere else in the whole of Sanskrit literature and are of highly uncertain meaning (*Trans. Philol. Soc.* 1946: 22–23). To provide these with Dravidian etymologies is as futile as to provide them with Indo-European etymologies, which Uhlenbeck did for the former (following Roth, presumably).

[22] The word occurs in Rig-Veda 10.155.1, an Atharvanic charm. It is common in later Sanskrit and has an abundant progeny ("blind" and "blind of one eye") in the Indo-Aryan vernaculars; see Turner, Ralph Lilley, *A comparative and etymological dictionary of the Nepali language*, London, 1931, s.v. kānu. Connection with an Indo-European root (s)qer- "to cut" or with qel- "to stick" is not obviously convincing. The Dravidian etymology is in Burrow, *Trans. Philol. Soc.* 1946: 22; earlier Gundert, *Ztschr. deutsch. morgenländischen Ges.* 23: 521. Not to be derived with Kuiper, F. B. J., *Proto-Munda words in Sanskrit*, 52, from a Proto-Munda *ga-ḍa "defective."

are accepted, this is evidence for the presence of Dravidian speakers as far towards the northwest as the Panjab, i.e. the upper Indus Valley, in the first centuries (it is uncertain how many) of the presence of Sanskrit-speakers on Indian soil.[23] It is not entirely clear evidence for the Dravidian nature of the Harappā language or of one of the Harappā languages; it does, however, lead towards that hypothesis.

If the Munda languages have been mentioned only in passing, it is not because there were no contacts between Munda speakers and those of the other two language families. The reason is that we know so little that is certain about the Munda family, either descriptively or comparatively. Even the most ambitious attempts at demonstrating borrowings from Munda into Sanskrit have been based on fantastically unsound methods. Although certain Sanskrit words with no Indo-European or Dravidian etymologies might be borrowings from Munda, it seems sounder to ignore the suggestions that have been made, until clear criteria for judging them are at hand (see further Appendix 2).

APPENDIX 1

SANSKRIT BORROWINGS FROM DRAVIDIAN

T. Burrow in *Trans. Philol. Soc.* 1946: 13–18 set forth criteria for identifying Dravidian words borrowed into Sanskrit. I summarize them,

[23] This thesis automatically rules out a recent hypothesis formulated by C. von Fürer-Haimendorf in *Proceedings of the 37th Indian Science Congress, Poona, 1950*, Part II: Presidential Addresses, 175–189, esp. 176–180, and New aspects of the Dravidian problem, *Tamil culture* **2**: 127–135, 1953. He bases his arguments on the results of excavation at Brahmagiri, in the Chitaldrug District of Mysore State, by Wheeler in 1947 (*Ancient India* **4**: 180–310, July 1947–January 1948; for the chronology of the site, esp. 200–202). The prior establishment of a precise chronology at Arikamedu (near Pondicherry) by Wheeler (*Ancient India* **2**: 17–124, July 1946) allowed a cross-dating for the Brahmagiri site and the establishment of this tentative chronology: Brahmagiri Stone Ax culture down to the beginning of the second century B.C., "continuing as a dwindling sub-culture through most of the succeeding Megalith phase"; megalithic culture "after *c.* 200 B.C. to the middle of the first century A.D., overlapping the Āndhra culture"; Āndhra culture from about middle of the first century A.D. to third century. The date for the beginning of the megalithic culture is an extremely tentative one. Wheeler pointed out, moreover, that a specific feature of the megalithic culture at Brahmagiri was the use of cists with "port-holes," that this site is near the northern limit of reported sites with this feature (Hyderabad is so far the farthest north) but that they are found in considerable numbers south to the tip of the peninsula, and that if it were not for the "wide and formidable disparity in date between the Indian cists and their Western analogues [Western Europe, 2500–1500 B.C.], a significant interrelationship could scarcely be questioned" (183). He added a reference to an early, unverified report of similar cists in the neighborhood of Karachi (301), desiderated a further search for megaliths in North India, and proceeded to speculate (303–304) on a possible Western origin for the Indian phenomena, with (so far as I can see) a plain warning that Iron Age megaliths of the Indian type are not forthcoming in the Western world at the right period and in the right place, but with the assumption of "the possibility of an integral connection between the port-holed cists of India and those of western Asia and Europe, in spite of the wide disparities of time and place" and an unwillingness to envisage independent origin of the port-holed cists "in regions which, however far apart, have long been interconnected by sea."

Fürer-Haimendorf in the two publications cited at the beginning of this note, and with more certainty and detail in the second than in the first, accepted with hardly a reservation the hypothesis that the megalithic culture was brought to South India from outside India towards the middle of the first millennium B.C. He finds it impossible to conceive that the megalithic, iron-using culture might have been a local development in some area of South India still unexplored archaeologically (as most of South India still is), with some stimulus perhaps from outside; I, not being an archaeologist or an anthropologist, am unable to combat his point of view with any authority. However, I am unable to accept his further hypothesis that the Dravidian languages, or their ancestor, were introduced into India by the bearers of the megalithic culture. He finds it impossible that the people with an iron-using, megalithic culture and the neolithic peoples overrun, could have spoken languages of the same family. But this *a priori* position, that peoples of different cultures cannot be linguistically related, is contradicted again and again in the Indo-European and in other fields—e.g. the relations of Latin, Celtic, and Germanic speakers within the Indo-European family in the time of Caesar and Tacitus are notorious. The general linguistic position taken by Fürer-Haimendorf is untenable, and with it falls his contention that North India could never have been Dravidian-speaking. To rule out the negative does not, to be sure, provide evidence that North India was ever Dravidian-speaking. But if Dravidian loanwords are found in the Rig-Veda, this is positive evidence which fills this particular vacuum most satisfactorily.

It is regrettable that Manfred Mayrhofer was persuaded (Die Substrattheorien und das Indische, *Germanisch-romanische Monatsschrift* **34**: 233–234 and fn. 9, 1953) by this tenuous archaeological hypothesis to abandon the position that he had taken earlier (Arische Landnahme und indische Altbevölkerung im Spiegel der altindischen Sprache, *Saeculum* **2**: 54–64, 1951) on solid linguistic evidence. This is not to say that one can agree with all details of Mayrhofer's earlier construction (e.g. too much weight was put on less than satisfactory Munda evidence), but at least in his theory Dravidian speakers were among the early peoples met by the invading Indo-European speakers. It is not quite clear what position he takes in the introduction to his *Kurzgefasstes etymologisches Wörterbuch des Altindischen* (1953), but certainly he holds that there are Dravidian loanwords to be found in the Vedas (10).

with some comment, as follows: (1) the Sanskrit word should have no Indo-European etymology —it should rather be: no certain or obvious Indo-European etymology; (2) there should be a wide currency of the etymon in the Dravidian languages and it should be a basic element in the vocabulary; (3) "a word is shown to be Dravidian in origin if it is clearly to be derived from some Dravidian root" (e.g. candana- "sandalwood tree, ointment" is Dravidian in origin since the corresponding Dravidian nouns cāntu, etc. are specialized derivatives of a verb meaning "to rub into a paste"); (4) the word should be of some antiquity in Dravidian (e.g. occurring in the earliest Tamil texts); (5) comparative lateness of appearance of the word in Sanskrit (or, one may add, appearance only or first in Sanskrit lexica) increases the probability of its being a borrowing; (6) in each case possible phonetic criteria should be looked for; (7) likewise, semantic developments can sometimes be taken as criteria. There should be added, possibly as a corollary to (2), that if the word denotes something peculiar to the Indian geographical or social scene, a Dravidian origin is more probable than an Indo-European one.

Not all these criteria can be brought to bear in all cases. Comparative simplicity and avoidance of the assumption of tortuous phonological and semantic developments should also be aimed at, following the general practice of all disciplines ("Occam's razor"), and may well at times tip the scales for borrowing from Dravidian rather than for an Indo-European etymology that has been suggested.

The etymologies that follow have been referred to in the body of this paper. The sigilla for the various languages and the bibliography are those given in *Lang.* **21**: 184, fn. 1, 1945, and *Bull. School Orient. and African Stud.* **15**: 98, fn. 1, 1953. The following short list gives the Dravidian languages in the order (essentially geographic) in which they are quoted, plus two other sigilla used in the etymologies: Ta. = Tamil, Ma. = Malayalam, Ko. = Kota, To. = Toda, Ka. = Kannaḍa (Canarese), Koḍ. = Koḍagu (Coorg), Tu. = Tuḷu, Te. = Telugu, Kol. = Kolami, Nk. = Naiki, Pa. = Parji, Oll. = Ollari, Go. = Gondi, Kui, Kuwi, Kur. = Kurukh, Malt. = Malto, Br. = Brahui; Skt. = Sanskrit, Mar. = Marathi.

1. Ta. kaital, kaitai; Ma. kaita; Ka. kēḍage, kēḍige; Tu. kēḍai, kēḍayi, kēḍāyi; Te. gēḍāgi "the fragrant screw-pine, *Pandanus odoratissimus*": Skt. ketaka-, ketakī- id. Burrow, T., *Trans. Philol. Soc.* 1946: 16: "the diphthong *ai* in the Tamil and Malayalam words is an indication that the word is originally Dravidian"; so also the suffix *-ai* in Ta.-Ma. and Tu.

2. Ta. ēlam "cardamom plant, *Elettaria cardamomum*"; ēla-v-arici "cardamom seed"; Ma. ēlam "cardamoms"; ēlatt-ari "cardamom seed"; Ka. ēl-akki, yāl-akki, yālaki "large cardamoms"; Koḍ. e·l-akki "cardamom seeds"; e·la male, e·latī male "cardamom plantation"; Tu. ēl-akki "cardamoms"; Te. ēla, ēlaki "cardamom plant"; ēlakulu "cardamom seeds": Skt. elā "cardamom." Kittel, no. 85.

3. Ta. palli; Ma. palli; Ko. e·paj; To. pagy; Ka. palli; Koḍ. palli; Tu. palli; Te. palli, balli "house lizard, *Lacerta gecko*": Skt. pallī, pallikā id. (lexical); Mar. palli, popular pāl. Kittel, no. 59; Emeneau, M. B., *Univ. of Calif. Publ. Class. Philol.* **12**: 261, fn. 31, 1943; Burrow, T., *Trans. Philol. Soc.* 1946: 10.

4. Ta. mayil, maññai; Ma. mayil; Ko. mi·l; To. mi·g; Ka. mayla, maylu; Koḍ. maylī; Tu. mairų; Pa. manjil, mañil; Oll. añgil; Go. mal; Kui meḍu, melu; Kuwi mellu "peacock": Skt. mayūra- id. (in two compounds in Rigveda 3.45.1, 8.1.25), mayūrī- "peahen" (Rigveda 1.191.14, an Atharvanic charm). Burrow, T., *Bull. School Orient. and African Stud.* **11**: 608–610, 1945. He discusses Przyluski's Austro-Asiatic suggestion (*Bull. Soc. Ling. Paris* **26**: 99 f., 1925) and shows that Skt. mayūra- is closer in form to the Dravidian series than to the posited Austro-Asiatic *marak or the like. He notes also that *r* in mayūra- instead of *l* is not entirely unexpected in Rigvedic Sanskrit. Further discussion by Jules Bloch, *Bull. Soc. Ling. Paris* **25**: 16, 1925. The suggestion (Walde-Pokorny **2**: 243) of an Indo-European etymology on the basis of connexion with Skt. mímāti "bellow, bleat," māyú- n. "bellowing," Gk. μμίζω "neigh," etc. is not convincing.

5. Ta. purru, purram "white anthill"; Ma. purru "ground thrown up by moles, rats, esp. a white anthill"; Ka. puttu, putta "white anthill"; Koḍ. puttī id.; Tu. puñca id., "snake's hole"; Te. puṭṭa "anthill, snake's hole, heap, lot, crowd"; Kol. (Kin.) puṭṭa "white anthill"; Nk. puṭṭa id.; Pa. putkal, (NE) puṭkal id., (S) putta "nest inside anthill"; Oll. puṭkal "white anthill"; Go. putti id.; Kui pusi id.; Kuwi [F] pūci "anthill," pūnja "ants' nest (earthen)"; Kur. puttā "anthill"; putbelō "white ants' queen" (belō id.); Malt. pute "anthill": Skt. puttikā- "the

white ant or termite"; pipīlika-puṭa- "anthill" (pipīlika- "ant"). Burrow, T., *Trans. Philol. Soc.* 1945: 111. Skt. puttikā- has been taken to be a meaning development from puttikā- "doll, puppet" by Boehtlingk-Roth and Uhlenbeck ("das puppenähnliche tier") and Monier-Williams ("so called from its doll-like form"); this is clearly *ad hoc*. Proto-Dravidian *rr survives in Ta.-Ma. but develops in two different ways (tt, ṭṭ) in different languages. Sanskrit has borrowed from two different Dravidian languages, giving puṭa- and *putta-, both meaning "anthill" or more specifically "white anthill," from the latter of which puttikā- is derived by a Sanskrit formative suffix.

6. Ta. nīr "water, sea, juice, liquor, urine, dampness, moisture"; (nīrpp-, nīrtt-) "become thin or watery (as liquid food in cooking), be wet, moist"; nīrmai "property of water, as coldness"; īr "moisture, wetness"; īram "wet, moisture, freshness, coolness"; īrali (īralipp-, īralitt-), īri (īripp-, īritt-) "become moist, damp"; īrippu "dampness"; īriya "damp, wet, cold"; Ma. nīr "water, juice, moisture"; īram "moisture, dirt"; īrikka "grow damp"; īrmam, īrman, īran "damp cloth"; Ko. ni·r "water"; To. ni·r "water"; i·rm "dampness"; Ka. nīr "water"; īra "moisture, dampness, wetness"; Koḍ. ni·rī "water"; Tu. nīruḷu "water"; Te. nīru "water" (literary); nīḷḷu "water"; īmiri "moisture"; Kol.-Nk. i·r "water"; Pa.-Oll. nīr "water"; Kui nīru "juice, sap, essence"; Br. dīr "water, flood-water, juice, sap": Skt. nīra- "water" (epic +); "juice, liquor" (lexical); nīvara- "water, mud" (lexical). Kittel, no. 157; Bloch, J., *Bull. School Orient. Stud.* **5**: 739, 1930; Burrow, T., *Trans. Philol. Soc.* 1946: 9. An old Indo-European etymology, that always required faith both in its phonology and in its semantics, has recently been revived with all the apparatus of the laryngeal hypothesis by Louis H. Gray, *Lang.* **25**: 376–377, 1949; it is hardly convincing *vis-à-vis* the general Dravidian word for "water."

7. Ta. nal, naṟ-, nalla "good"; nala (nalapp-, nalant-) "result in good, take a favorable turn"; nalappu "goodness, benefit, success"; nalam "goodness, virtue, beauty, profit, fame, prosperity"; nalavu "goodness"; naṟku "good"; naṟpu, narram, naṉpu, naṉmai, naṉri, naṉṉar "goodness" (some of these also mean "beauty"); Ma. nal "good, fine"; nalam, nallam "goodness, beauty"; nalpu, nalma, nanni, nanma "goodness"; nalla "good, fine, handsome"; Ko. nal va·yn "one whose mouth smells good, who enjoys food and prosperity"; na pal "teeth that grow straight and regular"; To. naṣ, naṣθ "beauty"; naṣo·n n. pr. of a man; Ka. nal, nalme "goodness"; naḷa "a good man; goodness, excellence, beauty"; Koḍ. nallë "good"; Tu. nalu, nala "good, cheap"; nalmè "goodness, friendship"; Te. naluvu "beauty, ability; beautiful"; Go. (M) nelā "good": Skt. Nala- n. pr. of a man. Emeneau, M. B., *Univ. of Calif. Publ. Class. Philol.* **12**: 255–262, 1943; this still seems to me to be a good etymology. Manfred Mayrhofer agrees, *Symbolae Hrozný* **5**: 371, 1950.

8. Ta. malai "hill, mountain"; Ma. mala "mountain"; Ko. mal im "buffaloes of the Nilgiri tribes (i.e. mountain buffaloes)"; mal a·ṛ "high downs on western half of Nilgiri plateau"; To. maṣ o·ṛ id.; Ka. male "mountain, forest"; Koḍ. male "thick jungle land, cardamom plantation in jungle on mountain"; Tu. malè "forest, hill overgrown with forest"; Te. mala "mountain"; Kol. ma·le "hill"; Pa. malaŋg "forest"; Br. mash "hill, mountain": Skt. malaya- "the mountains which border Malabar on the east (i.e. Western Ghats)" (epic +); "a celestial grove (Nandanavana)" (lexical); "a garden" (lexical); māla- "forest near a village; field" (lexical). Kittel, no. 164; also Gundert and Caldwell. The Sanskrit lexicographers derive malaya- from a hypothetical root mal "hold, possess," since the mountain range "contains sandalwood"! The commentators are wildly at variance about the meaning of māla- in Meghadūta 16, some taking it as a proper name of a field, others as a common noun; certainty seems impossible, but it is in all probability the word that we are dealing with, whatever its specific denotation in Kālidāsa's verse.

9. Ta. aiya excl. of wonder, of pity or concern; aiyakō excl. of pity or sorrow; ai "wonder, astonishment"; aiy-eṉal "uttering ai expressive of wonder, of distress or mental suffering, of assent"; aiyaiyō excl. of pity or grief; aiyō excl. of wonder, of pity or concern, of poignant grief; Ma. ayyā interj. of derision; ayyō, ayyayyō interj. of grief or pain; Ko. aya· excl. of surprise or grief; aya· ava· excl. of grief; To. eya· excl. of surprise; Ka. ayyō, ayyayyō, ayyayyē interj. of grief or astonishment or compassion; Tu. ayyō, ayyayyō interj. of grief, annoyance, or pain; Te. ayyo, ayyō, ayyayō, ayyayyō, ayayō interj. of sorrow, lamentation, pity, pain, etc.; Kui āige, āigo, āigōna, āike, āiko, āikōna interj. of annoyance, impatience, or disgust; Kuwi (S) ījaḷijō, ījalesa (j = y) "alas"; Kur. ayō, ayō ge excl. of

pain or surprise; Malt. aya, ayyi, ayyu excl. of wonder, woe, or joy; ay(y)oke, ay(y)okaboke "alas": Skt. aye excl. of surprise, recollection, or fear (esp. in dramas). In the Dravidian languages these are either vocatives of words for "father" (all the southern languages) or "mother" (Kui- Kuwi, Kur., Malt.), or have in a secondary way been equated with or assimilated to such vocatives; note Ko. aya· ava· "father! mother!" In Sanskrit there is no such connection. An Indo-European etymology might be suggested with such interjections as Gk. αῖ, αἰ, ἀιαῖ, Lith. aĩ, ái, German ei, and Skt. e, ai (both lexical) (*cf.* Walde-Pokorny **1**: 1), but the Dravidian forms are so much more current than the suggested Indo-European etyma that a borrowing from Dravidian seems more plausible.

10. Ta. kaḷam, kaḷaṉ "place, open space, threshing floor, battlefield"; Ma. kaḷam "threshing floor, level space for spreading grains for drying, battlefield"; Ko. kaḷm "place for threshing or dancing"; To. koḷṇ "threshing floor"; Ka. kaḷa, kaṇa "threshing floor, battlefield"; Koḍ. kaḷa "threshing floor"; Tu. kala "a square, bed of flowers, etc., place where pariahs assemble"; Te. kaḷḷamu "threshing floor"; kalanu "war, battle, combat, (B) threshing floor"; Kol. kalave "workshed in field, (Kin.) threshing floor"; Nk. kaḷave "threshing floor"; Pa. kali id.; Oll. kalin id.; Go. kāṛā "sacred enclosure, threshing floor"; Kui klai "threshing floor"; Kuwi (F) kṛanū, kalōmi id.; Kur. khall "field, piece of land suitable for tillage"; Malt. qalu "field on the hills": Skt. khála- "threshing floor, granary" (Rigveda +); "place, site" (lexical). Burrow, T., *Bull. Soc. Orient. and African Stud.* **11**: 133, 1943; *Trans. Philol. Soc.* 1946: 9. No Indo-European etymology has been plausibly suggested.

11. Ta. paṟu (paṟupp-, paṟutt-) "ripen (as fruits, grain), grow mature, arrive at perfection, become old, come to a head (boil), change color by age, become pale or yellowish (as the body by disease), become flexible, pliant"; paṟu, paṟuppu "ripeness, yellowness (of fruits), leaf turned yellow with age"; paṟunu (paṟuni-), paṟuṉu (paṟuṉi-) "grow ripe, become mellow, mature, be full or perfect"; paṟam "ripe fruit"; Ma. paṟukka "grow ripe, become well-tempered, suppurate, decay"; "a fruit put to ripen"; paṟuppu "ripening of fruit"; paṟuppikka "ripen artificially"; paṟam "ripe fruit, ripe plantains"; Ko. paṛv- (paṛd-) "(fruits) become ripe, (boil, sore) opens"; paṇ "fruit"; To. pošf- (pošt-) "ripen"; pum "fruit"; Ka. paṇ "be produced (ripe fruit)"; paṇ, paṇṇu "ripe fruit, ripeness"; Koḍ. paṇṇī "fruit"; Tu. palkuni, paḷkuni "be very soft (as an overripe fruit), be pliant, flexible"; parnduni "be ripe, mature, (hair) turns gray"; parndu "ripeness, ripe fruit, ripe plantains, ripe, gray"; palu "ripening (as of fruit), half-ripe"; Te. paṇḍu "ripen, mature"; "fruit, berry"; "ripe, mature"; paṇṭa "produce, crop, fruit, ripening"; Kol. panḍ- (panḍt-) "become ripe"; panḍuḍ "ripe fruit"; (Kin.) pan "fruit"; Nk. panḍ- "become ripe"; panḍe "ripe fruit"; Pa. panḍ- "(plant) matures"; parñ- "ripen"; pal "ripe fruit, pus"; Oll. paṛɳ(g)- "become ripe"; Kuwi (S) panḍu "ripe fruit"; Kur. pañnā (pañjā) "ripen, (boil) festers, have a yellowish appearance (as after a prolonged illness)"; pañjkā "fruits"; Malt. páne "ripen"; panjek, panjeke "ripe": Skt. phála- "fruit" (Rigveda +); phalati "bear or produce fruit, ripen" (epic +). Gundert, H., *Ztschr. deutsch. morgenländischen Ges.* **23**: 519, 1869; also Caldwell and Kittel; Burrow, T., *Trans. Philol. Soc.* 1946: 10; Ammer, Karl, *Ztschr. deutsch. morgenländischen Ges.* **51**: 128, 1948; Mayrhofer, Manfred, *Anthropos* **47**: 664, 1952; with some uncertainty Bloch, Jules, *Bull. Soc. Ling. Paris* **25**: 17, 1925; *Bull. School Orient. Stud.* **5**: 740, 1930; Kuiper, F. B. J., *Acta Orientalia* **16**: 305, 1938; the possibility is not even mentioned by Lüders, Heinrich, *Kuhn's Zeitschrift* **42**: 198 ff., 1909. This hypothesis of borrowing is much more obvious than the suggested derivations from Indo-European roots meaning "swell" or "burst" or than Sturtevant's connection with words for "leaf" (*Lang.* **17**: 6, 1941).

12. Ta. val "strong, forceful, skilful"; valam, vallam "strength, power, right side, victory, authority"; vallavaṉ, valavaṉ, vallāṉ, vallāḷaṉ "strong man, capable man"; vallu (valli) "be able"; vali "strength, power"; (valiv-, valint-; valipp-, valitt-) "be strong, compel"; valimai "strength, skill, hardness"; valiya "strong, big"; valivu "strength"; valu "strength, skill, ability"; (valupp-, valutt-) "be strong or hard"; valuppu, vaṉpu, vaṉmai, "strength, firmness"; valumai "strength, force, violence"; vala kkai, valaṅ kai "right hand"; Ma. val, valu, valiya "strong, powerful, great"; vallu "be able, strong"; valippam, valima "greatness, bigness"; valiyē "forcibly, suddenly"; valam "the right side"; valaṅ kai "right hand"; Ko. val "powerful, very, right"; val kay "right hand"; val(n) "man who is clever at cheating"; valc- "(man) becomes

stout, (heart) becomes bold, (grain) becomes solid lump when boiled"; To. pas "right"; pas koy "right hand"; paly- (pals-) "(child) becomes strong"; Ka. bal, bali "grow strong or firm, increase"; bal, balu, bolu "strength, firmness, bigness, abundance"; bala "right"; bala key/gey "right hand"; balume, balme, baluhu, balpu "strength"; Koḍ. bala "power, strength"; balate "right (hand)"; ballyĕ "great"; Tu. bala "strength"; balatu "the right side"; balata kai "right hand"; baiāpini "gain strength, recover health"; balike "prowess, strength, hardness"; balimè, balumè, balme "strength, might"; balu "very large, great, severe, violent"; Te. valāti "clever person, expert"; vala "right"; vala cēyi "right hand"; valadu "much"; valanu "skill, excellence, possibility"; "right, possible, convenient"; valan instrumental postposition; valamu "largeness, stoutness"; valūda "stout, large"; valla "possible"; vallaḍi "violence, oppression"; Pa. vela key "right hand"; Go. wāllē "much, very"; Kuwi (F) braiyū, (S) blājugatti (j = y) "strong"; Kur. balē, balēti "with the help of": Skt. bála- "power, strength, might" (Rigveda +); balena, balatas "by means of." The *v*-languages are primary; *v* > *b* in Ka. beginning in the 9th cent. A.D. Sanskrit may then have borrowed from a Dravidian language with secondary *b*. Kittel, no. 398; also Caldwell; Burrow, T., *Trans. Philol. Soc.* 1946: 19. Should we perhaps add also the name of the Rigvedic demon valá-, i.e. "the strong one"? It is just as good a suggestion as the derivations given from IE *wel- "drehen, winden, wälzen" or *wer- "verschliessen, bedecken."

13. Ta. mālai "garland, wreath, necklace, anything strung together"; malai (malaiv-, malaint-) "wear, put on" (Burrow, "put on as a garland," based on the old text Puṟanāṉūṟu 12, etc.); Ma. māla "garland, wreath, necklace, dewlap"; Ka. māle "wreath, garland, necklace"; Koḍ. ma·le "necklace, dewlap, jungle cock's ruff or neck feathers"; Tu. mālè "garland, wreath, necklace"; Te. māla id. Burrow, *Bull. Soc. Orient. and African Stud.* **12**: 390, 1948. He notes that Ta. has mālai in the oldest literature (Puṟanāṉūṟu 60, 76) and that the verb malai with a short vowel is just as old and related as a derivative from the noun. The Ka. word is classed by the Ka. grammarians as a *tatsama*, i.e. "a term not borrowed from Sanskrit, but existing in Kannaḍa as well as in Sanskrit" (Kittel's definition); this native analysis always may be taken as speaking in favor of Dravidian origin. The Ma.-Koḍ. meaning "dewlap" may possibly also be taken as evidence for mālai as a native Dravidian word. For non-Dravidianists it should be noted that the Dravidian forms are those that a borrowing from Sanskrit would have but are also those proper to a native Dravidian group of etyma.

APPENDIX 2

THE MUNDA LANGUAGES

Identification of borrowings from Munda into Indo-Aryan and into Dravidian engaged several scholars in the 20's and the 30's, notably J. Przyluski and Suniti Kumar Chatterji. This was continued by the publication in 1948 of F. B. J. Kuiper's *Proto-Munda words in Sanskrit* (Verh. d. K. Nederlandsche Ak. v. Wetenschappen, Afd. Letterkunde, N. R. **51.3**), a very ambitious work. In spite of a few tempting borrowings among those suggested by these various scholars (*cf.* Burrow, *Trans. Philol. Soc.* 1946: 3–6), there are several factors involved that give a conservative scholar pause.

First, the Munda languages are very poorly known to scholarship. Ambitious grammars and dictionaries have been published of Santali, Mundari, and So·ra· (Sabara or Savara), but, with all deference to the extended efforts and the devotion of the missionaries and the educationists who have worked here, we must judge, I think, that the intrinsic complexities of the languages of this family are not a suitable field for amateur effort. Extensive prefixing, infixing, and suffixing make both description and comparison of the languages very difficult, and until we have competent descriptions of several of them, comparisons concerned with borrowing or with genetic relationships within the family and outside it will be hazardous.

The second dubious factor concerning Indo-Aryan borrowing from Munda is that so far those who have tried to demonstrate such borrowings have been dealing not primarily with Munda but with Father W. Schmidt's Austro-Asiatic or with his Austric. Acceptance of the hypothesis that Munda, Khasi, Mon-Khmer, and various other languages of Southeast Asia form a language family to be called Austro-Asiatic can so far only be on a different plane from the scholar's attitude with respect to Indo-European, Finno-Ugric, Turkic, Bantu, Malayo-Polynesian, Algonquian, or Athabaskan. In contrast with these, the material with which Schmidt worked is badly recorded and analyzed,

and in spite of some tempting similarities, it is clear that little progress has been made beyond them in the search for phonetic correspondences. The field is one in which accidental similarities are still being found rather than etymologies. This scepticism becomes more pronounced when Austronesian (= Malayo-Polynesian) is brought in to form an Austric family. Archaeologists, historians and prehistorians, and ethnologists concerned with South Asia have erected lofty hypotheses on Schmidt's hypothesis, but the linguistic specialist should know full well what a weak foundation Schmidt has provided and should refrain from erecting his buildings on it.

In identifying Indo-Aryan borrowings from Munda, Austric material has been used to the fullest extent—groups of Austric etyma, including Munda words if they can be found in the badly recorded material, are provided as the bases for the borrowing. How hazardously speculative this procedure can be is clear from a glance at Kuiper's introduction in which are found these statements of his method: (6) "the Austro-Asiatic consonantal system had a relatively small number of phonemes with a wide range of possible realizations, the following sounds, for instance,

$$\frac{d/t \sim dh/th \sim r \sim j/c, s}{ḍ/ṭ \sim ḍh/ṭh \sim ṛ \sim {}^{*}ẓ/{}^{*}ṣ} \sim y \sim l$$

originally having constituted one phoneme"; (7) "another Proto-Munda phenomenon inherited from prim. Austric is the nasalization and prenasalization of the consonants of a root"; (7) "the vowels are largely interchangeable . . . the derivatives from one and the same root have often widely divergent forms; e.g. Skr. *tuṇḍa-* and *cañcu-* [both 'beak'] from *da-da*, *veṇī-*, *kabarī-* and *āpīḍa-* [the first two 'a braid of hair,' the third 'a chaplet tied on the crown of the head'] from *wa-da-*, . . . From the point of view of methodology this is, I think, the chief difficulty of these investigations, as by a purely mechanical application of the 'sound-laws' nearly everything can be demonstrated." If these are sound-laws (with or without quotation marks), we have got back to the days of Voltaire. No wonder Kuiper astonishes himself by the fact that "nearly everything can be demonstrated." Unfortunately, altogether too many of the Indo-Aryan borrowings from Munda astonish in this way. In addition, much derivation in the Austric languages depends on prefixation and infixation—though it is to be noted that little of this apparatus is so far known by satisfactory comparative methods. If words may be split up and prefixes and infixes discarded somewhat arbitrarily in the attempt to establish groups of etyma, this derivational method and the phonological method sketched above are together unlikely to establish much credence. But even so there are without doubt a few sane etymologies to be salvaged from the work already done, though I do not know how to identify them.

This slight critique of Munda and Austric studies makes clear, I hope, that the prime desideratum in this field is accurate and intensive recording and analysis of the languages concerned. It is only after this is accomplished that the way will be open for valid comparative work and for the establishment of a linguistic prehistory of the kind that is already possible for Indo-Aryan and Dravidian.

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